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SPACE AND SENSE-DATA *

W. RUSSELL BRAIN

I HAVE argued in the past that our knowledge of neurophysiology necessitates the adoption of some form of the sense-datum theory. The following are the chief reasons for this. I experience a sensation only when certain changes occur in the cells which compose my nervous system. When, by means of such sensations, I perceive an object outside my body, the sensations are the end-result of a long chain of physical causes starting at the object and ending in my nervous system. When what I perceive is in my own body, the same is true, except that the chain of physical causes begins at the surface of, or within, my body instead of outside it. In either case, what finally happens in my nervous system at the moment when I perceive either an external object or my body is physically quite unlike the initial change which causes it. There is, for example, no resemblance between the light-waves which originate in the sun or are reflected from a table, and the complex electrical changes which occur in my brain when I see the sun or a table. A sense-datum, therefore, if we use that term to describe a colour, a sound, a touch, or a smell which we experience, is caused by the object which we see, hear, feel or smell, but cannot be either a part of that object or qualitatively like it. Moreover it is argued that, since in perceiving we are dealing with a chain of physical causation which takes time, we must always perceive an object at a time later than that at which the series of physical events which leads to its perception starts from it. This time is only a fraction of a second when we are concerned with awareness of events in our own bodies, or with seeing an object on the surface of the earth, but it may be appreciable when we are dealing with the sound made by a rapidly moving object such as an aeroplane, which sounds as if it were in one place when we can see it in another, or when light travels from

* Read to the British Society for the Philosophy of Science, 30.xi.59

some celestial body to the eye. In the case of the sun, for instance, it takes about eight minutes for a light-wave to reach us. Hence it is argued that the only information which sensation can give us must always be about the past state of the object perceived. This makes no practical difference in everyday life because the objects with which we deal change so little from moment to moment, that for practical purposes their present state, at the moment at which we perceive them, is identical with that at which the light-wave or sound-wave left them. In the case of astronomical objects, however, the time factor is of practical importance, and has to be taken into account. If the light-waves from a distant star take several thousand years to reach us, we should not know if it had become extinct several hundred years ago. In what sense, therefore, can we be said to be seeing the star; and must we not conclude from this temporal argument, that once again our sense-data cannot be identical with the object which excites them?

This view of the purely causal relationship between object and sense-data leads to the question, where are sense-data situated? Visual sense-data are situated in a space in which they are related to each other, and to the body of the observer. Sounds are on the whole less sharply located; nevertheless sound is in, or comes from, a certain direction in most instances, and the directions from which sounds come can be distinguished from one another. Smells are related to space in a more diffuse way, but they also have spatial reference. Some of my bodily feelings are very precisely located, others more diffusely, but they also are all somewhere.

If we become aware of all these sense-data as the result, and solely as the result, of changes occurring in the brain, which is inside the skull, how do they come to be experienced either outside the body when they are related to external objects, or within the body itself, but never within the brain? Some thinkers, chiefly physiologists and psychologists, have tried to get over this difficulty by invoking a process, which they term projection. Sense-data, though depending upon physiological changes in the brain, are held to be projected to the spatial position in which they seem to be located. No-one has ever succeeded in describing how projection is brought about, and on examination it seems to be no more than an imaginary process invoked in an attempt to describe how changes within the brain lead to an experience in which a sensory quality is located outside the brain.

Some writers have sought to get over this difficulty by supposing that sense-data or perceptual objects exist in one space and physical

objects in another. (I do not think it necessary here to discuss the relationship between sense-data and perceptual objects, because I do not think it has been suggested that they might be located in different spaces.) Since Bertrand Russell has been the chief contemporary exponent of this view, I shall illustrate it from his exposition.

> Naïve realism identifies my percepts with physical things; it assumes that the sun of the astronomers is what I see. This involves identifying the spatial relations of my percepts with those of physical things. Many people retain this aspect of naïve realism although they have rejected all the rest. But this identification is indefensible. The spatial relations of physics hold between electrons, protons, neutrons, etc., which we do not perceive; the spatial relations of visual percepts hold between things that we do perceive, and in the last analysis between coloured patches. . . . When I say that something is 'outside' me, there are two different things that I may mean. I may mean that I have a percept which is outside the percept of my body in perceptual space, or I may mean that there is a physical object which is outside my body as a physical object in the space of physics. Generally there is a rough correspondence between these two. The table that I see is outside my body as I see it in perceptual space, and the physical table is outside my physical body in physical space. . . . One of the difficulties which have led to confusion was failure to distinguish between perceptual and physical space. Perceptual space consists of perceptible relations between parts of percepts, whereas physical space consists of inferred relations between inferred physical things. What I see may be outside my percept of my body, but not outside my body as a physical thing. Percepts, considered causally, are between events in afferent nerves (stimulus) and events in efferent nerves (reaction); their location in causal chains is the same as that of certain events in the brain.[1]

It would seem to follow from this (and it is a view which I have myself adopted in the past), that each observer has his own private perceptual space, whereas there is a physical space common to all observers. My private perceptual space is the space in which are located all the sensory characteristics of everything I perceive, that is objects outside my body, and my body itself, and all that I can perceive as happening within it. Every person is believed to have such a perceptual space private to him. This idea has both advantages and disadvantages. One of its advantages is that it provides a habitat for

[1] Bertrand Russell, *Human Knowledge: its Scope and Limits*, London, 1948, pp. 217-225

hallucinations, which are private to the observer, and yet appear to exist in spatial relationship with the world of external objects which he perceives. If one observer says that he sees a man walk through a room, in which other people present say they see nothing, this is not difficult to understand if we believe that the sensory qualities of the objects in the room and their spatial relationships are in some sense private to the observer. The hallucinatory appearance of the man, though resulting from some abnormality in his nervous system, is not to be regarded as an event of a different perceptual status from his awareness of the normal occupants of the room. Similarly if a person who has had a limb amputated experiences a phantom limb and says that it feels as though the limb was still there, his imaginary limb is located in a spatial relationship to the rest of his body and may be said to exist in his private perceptual space though it does not exist any longer in physical space.

There are, however, certain disadvantages in the idea that we each of us possess a private perceptual space. The first problem which it raises is the relationship of these multiple private perceptual spaces to one another and to physical space. Russell clearly believes that my private perceptual space is in my brain. It cannot, however, be in my perceptual brain, which is situated in my perceptual space within my perceptual head. It must therefore be in my physical brain, and that raises the question of the relationship between my perceptual space and physical space. A disadvantage, which seems to me more serious, is the complication which it introduces into the simplest events of everyday life. In order that I may lift a cup of tea to my lips, I must first by sight and touch become aware of the position of the cup as a perceptual object in my perceptual space, in which also my perceptual hand and mouth are situated. I then go through a series to movements in that space guided by my sense-data of touch, pressure, and vision, as the result of which I succeed in bringing the perceptual cup to my perceptual mouth, and I am gratified to find that although all this has taken place in a perceptual world located within my physical brain, I have succeeded in lifting the physical cup with my physical hand to my physical mouth.

The sense-datum theory, and therefore the concept of private perceptual spaces based upon it, has to meet criticism of a more general kind, namely that it is self-contradictory. This criticism, which has been expressed by a number of philosophers, is well summarised by Hirst as follows.

> If we, as minds, never directly perceive material objects but are only directly aware of images or mental representations allegedly caused by them, how do we know that there are any material or physical causes or what their nature is? We cannot look behind the barrier of ideas to see what their causes are like, if they have any. Locke . . . failed to notice that his theory is self-refuting, for its conclusion contradicts the premise assumed in physiology that we do perceive material things such as sense organs and brains.[1]

I believe that the facts of physics and physiology which I have outlined necessitate some form of the sense-datum theory, but I have come to think that it is possible to retain this without the necessity of believing in a multiplicity of private perceptual spaces. I think that a revised sense-datum theory, in the form in which I propose to restate it, may go some way towards reconciling the representationalist view of perception with the views of some at least of the realists.

In an earlier discussion of this question I said:

> If at least what the philosophers call secondary qualities, such as smells, sounds, colours and so on are quite unlike the physical stimuli which give rise to them, then we must regard them as symbols of physical reality and say that the receptive function of the cerebral cortex is to provide us with a symbolical representation of the whole of the external world, not only distinguishing objects by their qualities, but also conveying to us the spatial relationships which exist between them, and at the same time giving us similar symbolical information about our own bodies and their relationship with the external world. All this information, of course, is given us not merely for the sake of pure awareness or contemplation, though that may sometimes be a by-product of it, but in order that we may act; hence it is linked, in ways with which we are familiar, with the motor activities of the brain.[2]

And Russell said:

> Percepts, considered causally, are between events in afferent nerves (stimulus) and events in efferent nerves (reaction); their location in causal chains is the same as that of certain events in the brain.[3]

Both of these accounts recognise the importance of receptive and motor functions and, by implication at least, the dependence of the latter upon the former. Russell goes further in locating percepts at regions of the brain regarded causally as between afferent and efferent

[1] R. J. Hirst, ' Perception, Science and Commonsense ', *Mind*, 1954, **63**, 388

[2] W. Russell Brain, *Mind, Perception and Science*, London, 1951, p. 4

[3] Russell, op. cit., p. 225

pathways. Percepts are thus treated, both as experiences, and in respect of their physiological substrata, as isolable from their motor accompaniments or sequelae.

No doubt we habitually consciously isolate the perceptual elements in an experience from our actual or potential actions with regard to them, and this has led to the belief that percepts exist as pure sensory events, and the neurophysiological basis of which must therefore be at the termination of the relevant afferent sensory pathways, and proximal to the neural substratum of any motor activities to which they may give rise. I suggest that both sense-data and percepts are abstractions which, however convenient for many practical purposes, are misleading when we come to consider the nature of experience, either epistemologically or physiologically; that experiences which possess nothing but sensory characteristics, do not exist; that what we mean when we talk about sense-data and percepts is something different; and that, whatever it is, it is not to be found stuffed into a crevice between sensory and motor nerve-pathways in the brain.

If we adopt the sense-datum theory in the form proposed by Russell, the whole of the space which I perceive exists in my physical brain. Hence when I lift a teacup to my lips, the sight of the cup and of my hand, the pressure and weight of the handle on my finger and thumb, the sensation of the movement of my arm, and the subsequent pressure of the cup upon my lips, together with the spatial relations of these various experiences, are all located within my brain. My success in performing the manoeuvre depends upon the fact that the various brain events which cause me to have the relevant experiences represent corresponding events in the physical tea cup and my physical body in such a way that when I experience this series of sensations these physical events are taking place. This leads to the central part of the view being put forward in this paper.

Whatever view different schools of philosophy may take about sense-data, no-one, as far as I know, has denied that in normal, as opposed to hallucinatory perception, such physical events are taking place. Hence when I lift a cup to my lips I know both where the cup is, and where my lips are in physical space. If this be conceded, it follows that our sense-data may give us information about the spatial relationships of external objects both to one another and to our own bodies. This information guides, and is validated by, our motor activities in relation to such external objects. This enables us to dispense with the idea of a multiplicity of private perceptual spaces, one for each

observer. As far as the physical world is concerned, there is only one space, in which physical objects, amongst which are our own bodies, exist. The spatial elements in perception give us information about the spatial relationship of bodies in physical space. What is private and subjective about perception is that this information is individual to each one of us, and presents itself in the form of a perspective, to use another of Russell's phrases, centred upon our own bodies.

This view of perception is not inconsistent with the belief that the sensory qualities of objects, colours, sounds, smells, and touches, are the products of the activity of our nervous systems, and qualitatively unlike the objects which are their remote and initiating causes, but the spatial characteristics of the objects I perceive, such as their size, shape, and relative position, the spatial characteristics of my own body, and the spatial relationships between external objects and my body, are the characteristics and relationships of those objects as physical objects. Unlike other sensory qualities, they are not created by my nervous system, the function of which in this respect is merely to give me information, that is, to make me aware of them.

How on this view are we to deal with the criticism derived from the sense-datum theory, that the time factor involved in perception means that what we are aware of can never be the physical object perceived? The answer to this is that if being aware of objects is identical with receiving information about them, the time interval involved in perception does not impair the accuracy of the information as far as normal perception of most terrestrial objects is concerned. When I see an object, even a moving object, in spite of the speed of light and the time taken for the transmission of a nerve impulse from my retina to the visual area of my cerebral cortex, the object really is where I see it. The same is true of an object held in my hand; for the time taken by the sensory nerve-impulses to reach my brain does not render the information about the size, shape and position of the object inaccurate. The test of the motor response can be applied in each case. The information is reliable as a guide to movement in respect of both the distant object and the object grasped. When we come to astronomical objects, the time factor involved in the conduction of light waves may be great enough to make a practical difference. Now, however, we are dealing with information about the object's past, and this becomes inaccurate only if we wrongly suppose that it relates to the object's present situation. Even in such cases, however, we may have to introduce a qualification. When I speak about shading

my eyes from the sun, I do not normally mean the astronomers' sun which is by now in a slightly different position from the sun which I see. For practical purposes the sun, as the object of my perception of light and heat, is equivalent to the light and heat waves during the last part of their passage through the atmosphere, and my sensory information about the sun as an object in this sense is accurate. Similarly, when I look at a star which may be situated many millions of light years away, unless I am astronomically sophisticated I shall not even know of the existence of the astronomers' star, but I shall point correctly to the physical object composed of the light waves from the star during the last part of their course before they reach my eye. In these instances again the accuracy of the information is to be measured by that of the motor response, i.e. shading the eyes from the sun and pointing to the star.

Here we have the answer to the objection to the sense-datum theory made by Hirst and others. As I have said elsewhere,

> If we start our description of perceptual knowledge with a subject and an object, we shall rapidly get into difficulties from which there is no escape. The fact with which we must begin is the fact of knowledge, experience, or information, if we use the last word, as I think we must, to imply a receiver as well as something received. If we start with knowledge or experience we start with the subject-object relationship already given. We do not need to ask how we become aware of things outside ourselves because it is with that awareness that we begin. It is easy to fall into the error of supposing that the sense-data generated by the brain must be *entirely* unlike the physical objects which, by acting upon the brain from the outside world, produce them. But, with some important exceptions, this view is mistaken. . . . One of the most important functions of the brain is to provide us with an accurate representation of the spatial structure of the external world as well as of our own bodies. An important part of this structure is the ever-changing relationship between our bodies and their environment: awareness of the externality of what is outside our bodies is therefore given in all ordinary acts of sense perception.[1]

Hence while there may be forms of the sense-datum theory which imply that, in Hirst's words, ' we, as minds, never directly perceive material objects but are only directly aware of images or mental representations allegedly caused by them ', and in respect of which it is legitimate to ask, ' how do we know that there are any material or

[1] W. Russell Brain, *The Nature of Experience*, 1959, pp. 31-32

physical causes, or what their nature is?' this is not true of the form of the theory now proposed. In that we do have direct awareness of external objects, and of our own bodies. We do perceive material things such as sense organs and brains. We can measure distances, infer from our observations the speed of light and of the nerve-impulse, and learn from physical and physiological facts, particularly the structure and activity of the nervous system, that some sensory elements are the products of the nervous system. If our minds had no information concerning material objects, we could never know that material objects were the causes of our sensations: if, however, we have sufficient information concerning the structure of material objects, there is nothing illogical in inferring that some elements in our perceptions are subjective. All that is needed to validate this inference is the power of spatial and temporal discrimination. Whether we discriminate objects such as pointer-readings because they depend upon black and white contrast or contrasting colours may be quite irrelevant to the accuracy of the observation.

It may be said that this does not meet Hirst's objection, for, if our knowledge of the structure of the external world depends upon sense-data which are generated by our brains, and are therefore subjective, we cannot know that there are such things as external objects to cause our sense-data. It is necessary, therefore, to examine the relationship between structure and sense-data. The fact that structure is independent of some sense-data is an everyday experience. We recognise a photograph although it is a flat, black and white representation of a coloured three-dimensional object, and we recognise it as a photograph of the same object whether it is large or small. Similarly we recognise the structure of a word, whether it is printed in black on white or blue on red. The distant hills look small and blue, and as we approach them they become large and green, but we recognise them as the same hills. In order that we may recognise a spatial structure visually, we must be able to discriminate perceptual differences, but within a wide range it does not matter what shades are used to differentiate them, provided the shape of the boundary between them remains the same. Hence, provided an area of the body surface, such as the retina, possesses receptors which are stimulated from different points of space, and the nervous system can discriminate the nervous impulses excited by such stimuli, we possess the basis of visual discrimination of spatial structure in the external world, and the fact that this discrimination reaches consciousness literally coloured by

sense-data, which themselves do not exist in the external world, does not mean that the information they mediate is not true information. Most mammals do not possess colour-vision, which is limited to insects, birds, the higher primates, and man. This means that those organisms which do possess colour-vision have evolved a nervous system capable of responding differentially to differences in the wave-length of light. Those without colour-vision can still discriminate objects by vision, but those with colour-vision possess greater powers of discrimination.

Thus we arrive at the view that in perception we have direct knowledge of the size, shape, and spatial disposition of external objects. What we know about them in these respects is, subject to accidental errors, genuine knowledge. They are where we suppose them to be in a public physical space. Our knowledge of this is personal and private, but that does not mean that we create a private perceptual space in which our perceptions of external objects exist. This holds true broadly of terrestrial objects, with which we ordinarily deal. When we come to celestial objects, we must either interpret object as meaning the last phase of the energy they propagate before it reaches our bodies, or we must say that the spatial characteristics of which we have present knowledge are those of their past, and often their very remote past.

To understand the part played by sense-data in perception, it is necessary to look at the problem created for the living organism by the physical stimuli reaching it from external objects, and its need to develop a capacity to respond to them. The physical stimuli are extremely diverse, comprising as they do light-waves, temperature-changes, sound-waves, the presence of particular molecules in the air or in food substances, and the physical changes directly produced by the pressure of objects on the body, the movements of its parts, and gravity. The organism must develop specific receptors which are selectively responsive to each variety of physical stimulus. All that such receptors can do when stimulated is to excite nerve-impulses, which consist of electrical changes in the nerves running from the receptor to the central nervous system. These electrical changes differ somewhat in frequency and rate of conduction, but within a comparatively small range. There is no evidence that any qualitative differences in the nerve-impulses from particular receptors correspond to the very wide range of differences between the physical stimuli. All our awareness of perceptual difference therefore depends upon

differences in the spatial and temporal configuration of the electrical changes reaching the brain. When such changes occur, we experience the appropriate sensation, and it is arguable that the occurrence of the electrical changes is the experiencing of the sensation.

It is inevitable therefore that a percept should contain subjective elements, and, as we have seen, it is these which make perceptual discrimination possible. External objects, therefore, are perceived *through* the nervous system, and such of their physical differences as can be distinguished by our sensory receptors are represented by distinguishable sensory qualities of sense-data. We are familiar with the fact that the sensory characteristics of objects are modified by the medium through which light-waves pass on their way from them to us. The colour of distant objects is affected by the state of the atmosphere, and if I look at the world through rose-coloured spectacles, objects will have a rosy appearance. This process of *labelling* physical objects with sensory qualities in order to discriminate their physical properties would be useless if the label were not attached to the object, and this is what happens in perception. We have accurate spatial information concerning the external world, as an inherent part of which it is perceived with sense-data which symbolise some of its physical properties. The same is true of our own bodies.

Thus, although it is sometimes convenient to talk about pure sense-data, and the localisation of sensory pathways and centres in the nervous system, this is an abstraction from a more complex and complete totality. Quite early in infancy the visual appearance of an object stimulates the young child to make movements with its arms in the direction of the object. If it succeeds in grasping it, vision is reinforced by sensations of touch, and knowledge of the direction of the object becomes linked in an elementary way with knowledge of the movements required to grasp it. So perception is from the first sensori-motor, and sense-data through experience may come to convey also other information related to other potential sense-data as of texture, weight, etc.

Much of this may not be obvious even on introspection, but it has at least originated in conscious experience. Physiology shows us that perception is profoundly influenced by factors which do not as such enter consciousness at all. The main evidence for the part played by these unconscious afferent impulses is the profound disorganisation of perception which occurs when they are themselves disorganised or cut off. For example, our whole perceptual orientation in space depends

upon the continuous reception of nervous impulses from the labyrinth, yet we are completely unaware of these as such. Hence not all perceptual information is conscious information in the sense that we are aware of it as constituting an individual element in what we perceive, like a colour. Nevertheless it contributes to the totality. This alone illustrates the inadequacy of any philosophical account of perception which is limited to the untutored observations of the percipient, however introspective.

Finally there is a question which is fundamental for our understanding of perception, and indeed of consciousness itself. In 1946 I wrote:

> Let us suppose that we are watching an observer looking at a circle. There is a sense-datum or a group of sense-data which are circular. Light waves, which in their grouping maintain a circular relationship, travel from the object to the eyes of the observer. On a circular area on each of his two retinae disturbances are set up which excite nervous impulses which travel through his optic nerves, tracts and radiations to the visual areas of the cerebral cortex. Only when the nervous impulses reach the cortex does the observer see the circle. If physiological idealism is true we might expect to find that there is something circular about the events at the cerebral cortex, for it is these, we are told, which are ' projected ' on to the outside world when we perceive a circle. Nothing of the sort is true. The area of cortical excitation which exists when we perceive a circle is divided into two halves, one in each cerebral hemisphere. Pathways connecting them exist, but these appear to play no part in our perception of the two halves of a circle as one whole, for this still occurs when the connecting pathway (the corpus callosum) is divided.[1] Neither half is semicircular; it is roughly the shape of ⊃, the closed end lying in front and the open end behind. The right half of the circle is represented in the left cerebral hemisphere and vice versa, and the lower quadrant is represented above the upper. There is another complication. We saw that a circular nervous disturbance occurred in each retina; thus each half of the circle has a double representation in the nervous system, and, though the pathways for the two corresponding halves—that is, the two right halves and the two left halves—come to be close together behind the optic chiasma, and are represented in the same area of the cerebral cortex, there is no anatomical point at which they fuse, as Sherrington[2] demonstrated in

[1] A. J. Akelaitis, *Arch. Neurol. Psychiat.*, Chicago, 1941, **45**, 288

[2] Sherrington, *The Integrative Action of the Nervous System*, London, 1906, pp. 354 sq.

his experiments with flicker. Finally, since the two halves of the circle are represented in cortical areas lying parallel to each other, the cortical disturbance is three-dimensional.

Thus when we perceive a two-dimensional circle we do so by means of an activity in the brain which is halved, reduplicated, transposed, inverted, distorted, and three-dimensional. If physiological idealism is to be really physiological it must admit that its theory of projection breaks down because the circle which is said to be projected from the cerebral cortex never existed there at all.[1]

To-day I should qualify and extend this statement. The visual cortex is a necessary condition of vision, for its destruction renders the subject blind, but the same is true of the optic nerves. Why, then, do we attach importance to the cortex? Because it is the last anatomical point on the afferent visual pathway of which this is true. But it does not follow that the function of vision is *located* there. It may be inaccurate to speak of its location in any particular part of the nervous system. If the brain largely acts as a whole, vision may be a property of the whole of it, in which case it is represented in the brain as a whole in a different way from its representation in the visual cortex. The latter then becomes only the last point at which nerve-fibres solely concerned with vision exist as an isolable group. The same may be true of other sensory pathways, and of the motor cortex, and motor pathways too. If so, the fact that there is nothing circular about the activity of the visual cortex when we see a circle becomes part of the larger question: is there anything circular about any of the brain's activities in these circumstances? Is neuropsychological isomorphism a necessary concept, or indeed a meaningful one? When we perceive a circle there is always something circular in the body (I am not now discussing tilted circles). If I see a circle there is a circular disturbance on my retinae. If I feel a penny pressed into my palm there is a circular disturbance in the skin of my hand. The information concerning the circularity of the visual or tactile object is conveyed along the relevant nerves in a complex pattern depending upon the particular nerve-fibres excited, the rate and frequency with which impulses pass up them, and the temporal pattern of their individual excitation. In other words it is coded. Up to and including the cerebral cortex there is a detectable correlation between a nerve fibre and the point on the retina or skin excited, as shown by the sensation aroused by stimulating it and the defect caused by its destruction. Beyond this there is

[1] W. Russell Brain, *Philosophy*, 1946, **21**, 133

no such correlation detectable. Somehow this coding of spatial information in a spatio-temporal pattern of frequencies is decoded into a static representation of a spatial pattern.

How this is done goes to the heart of the problem of the brain-mind relationship. We do not yet know enough to describe it in detail, but we can see at least the outline of the process. Consider some simple facts. I ask someone to close his eyes and stretch out his arm, and by moving his extended forefinger to describe a circle in space: next, keeping the forefinger still, but moving the wrist, to describe the same circle, by which I mean a circle in the same place: and, finally, keeping both the finger and wrist still, to do it again by moving the arm at the shoulder. No one has any difficulty in doing this, nor, in doing so, doubts that the three circles are, approximately at least, *in the same place*. What does this involve in psychological and neurological terms? Psychologically there is initially the decision to carry out the movement, which in some people will be accompanied by a visual image of the circle to be described. Neurologically, a succession of motor impulses will cause contractions of the right muscles in the right order to produce the desired effect. For each of the three movements the muscles will be different, and the parts of the motor cortex of the brain excited will differ correspondingly. Psychologically, while the movement is occurring there will be awareness of its circularity, derived (neurologically) from sensory receptors chiefly in and around the joints, which again will be different in each case. The resulting sensory impulses will reach different regions of the sensory cortex and there will be nothing circular about their disposition there. The nervous system, then, must have the means of extracting from these completely diverse sources the conscious information that the tip of the finger is describing a circle in the same place, just as on the motor side it is able to use different groups of muscles to the same end. We may note in passing that to describe one circle involves the retention in memory of the earlier part of the movement as a guide to the rest, and that memory is also required to enable the second and third circles to copy the first. But the process is much more complex than that, for the three successive movements would not describe circles in the same place unless the body had meanwhile remained stationary, so all the sensory and motor pathways and muscles concerned in the maintenance of the posture of the body have played an unconscious part in the procedure. And ' the same place ' has meaning only in relation to the rest of the space in which the body and external objects are situated,

so the conscious and unconscious factors contributing to awareness of space are also involved. (No one, for example, could carry out the procedure accurately if as the result of an attack of giddiness the room appeared to him to be going round.) Viewed in isolation the sensorimotor substrata of the three movements are different, but when this information is correlated with that from the whole body (the so-called ' body-image ') the result is awareness that the movement of the fingertip is the same in each case. Hence, awareness of the circularity of the movement is the product of a very complex neurological integration of information in space and time.

Pötzl[1] points out that as in mathematics a property which remains the same in spite of its transfer from one co-ordinate system to another is termed an invariant, so the cerebral activity, which produces awareness of a single space out of nervous impulses from many sensory sources, does so by creating invariants. A similar activity is involved in our recognition of the identity of visual objects independently of the relation system in which they are at any moment arranged, and, one might add, in spite of the perceptual changes produced by changes in their distance from the observer and perspective. The creation of such invariants is the function of the schemas, whose role in perception and speech I have discussed.[2]

So when we see a circle, the fact that the pattern of cells excited in the visual cortex is not itself circular is of no functional significance. All they need to do is to transmit information in the spatio-temporal form of electrical impulses for integration at higher levels of the nervous system. We do not at present know how that process makes us aware, either that we are seeing something, or that what we are seeing is circular.

[1] O. Pötzl, *Die Aphasialehre von Standpunkte der klinischen Psychiatrie*, Bd. 1, *Die optisch-agnostischen Störungen*, 1928

[2] W. Russell Brain, 'The Concept of the Schema in Neurology and Psychiatry' in *Perspectives in Neuropsychiatry*, ed. by D. Richter, 1950, p. 127; ' Aphasia, Apraxia and Agnosia ', in *Neurology* by S. A. Kinmer Wilson, ed. by A. Ninian Bruce, 1955, 3, 1413.

HISTORY AND PHILOSOPHY OF SCIENCE IN BRITISH COMMONWEALTH UNIVERSITIES *

W. Mays

1 *The General Problem*

It has been evident for some time that it would be useful to have a picture of the way 'The History and Philosophy of Science' was developing as a subject in the universities of the British Commonwealth, both in its teaching and research aspects. These universities form a fairly coherent unit since in most cases, with divergencies here and there, they have been founded on the model of British universities.

In order to obtain this information a questionnaire was sent out to 100 universities, most of which were in Australia, Canada, India, and South Africa. Sixty-nine replies were received. Out of these thirty-nine reported activity in the history and philosophy of science in their respective universities. The others were negative, either reporting no activity or returning the questionnaire without comment. The questionnaires were sent out in January 1959 and most of the replies had come in by the Easter of that year. In order to supplement the information given by the questionnaires, further enquiries were directed to individuals teaching the subject at various universities.

Six groups of questions were asked in our questionnaire: (1) was the university teaching the history (and philosophy) of science as a regular and independent part of the curriculum; (2) whether there was an established post or the expectation of one; (3) the type of course given; (4) the manner of treatment; (5) the value of the subject; (6) general remarks. The columns of our tables cover the most important of these headings.

This survey was appreciably helped by the fact that a little earlier in the autumn of 1958 an enquiry had been made into teaching and research in the history and philosophy of science in modern English universities, to which questionnaires had been sent. This had been followed by a conference in the November of that year at Leeds,

* This survey should be factually correct up to 1st March 1960.

where the results of the questionnaires were discussed. In its essentials the discussion covered (1) the content and purpose of university courses in the history and philosophy of science, (2) ways and means of increasing and improving teaching in this field in modern universities. Various recommendations were made for stimulating an interest in the subject and these have still to be acted upon.[1]

We may summarise some of the main conclusions of the Leeds conference. It was felt that there was a need to create more specialist posts in the subject. University teachers were needed for (*a*) research, (*b*) undergraduate teaching, (*c*) post-graduate teaching. There was a need to include the history and philosophy of science as part of the general education of sixth-formers as well as undergraduates. There was also a need to provide courses in the departments of education as the demand for the history and philosophy of science in schools will probably expand. A report of the Science Masters Association has stressed the importance of developing teaching in this subject.

As the conference saw it, the introduction of the subject in university curricula has three main functions: (1) the advancement of knowledge; (2) general education; (3) the training of teachers.

Among the more general points made was that few historians turned out by history departments have sufficient interest in and knowledge of science to specialise in this field. The consensus of opinion seemed to be that the history and philosophy of science provides a way of introducing science to arts students that may be more effective than instruction in one science. It gives them opportunities to become acquainted with different modes of thought and for mixing with students of different faculties. The conference also was of the opinion that teachers in the history and philosophy of science need to have a knowledge of both subjects, and that courses in the history of science must either include or be accompanied by instruction in the philosophy of science.

No strong opinions were expressed as to whether the subject was to be taught in a separate department or as part of a larger department. However, as we shall see below, Melbourne in the light of its own experience is strongly in favour of an autonomous department. No doubt the success in the past of the post-graduate department at University College, London, is to be traced to this fact. In a modern university, unless a subject has departmental independence and representation on the governing academic body, its chances of internal growth

[1] Professor S. E. Toulmin and his colleagues carried out the Leeds enquiry, whilst Professor P. H. Nowell-Smith reported on the conference.

may be slight. One of the reasons why the subject has not grown commensurately with its educational value, is that it has usually had to be a subsidiary enterprise of one or two interested people who have not always been strongly supported by their own department.

Our own questionnaire was modelled on the Leeds one. This was done to make our survey comparable with the results of the previous investigation. Our enquiry therefore incorporates the Leeds data as far as modern English universities are concerned. We have also included one or two universities not covered by the Leeds survey.

The Commonwealth universities, with the exception of Melbourne, showed a similar pattern of activity to the British ones. On the whole, though most universities are aware of the importance of the history and philosophy of science, very few independent appointments have been made in this subject. Most of the lecturing therefore tends to be of a stop-gap variety. It is carried out by members of science, philosophy, or education departments working individually or as a team.

Scientists seem more aware of the need for instruction than philosophers, and philosophers more than historians. As a general rule science departments are more inclined to interest themselves in the history rather than the philosophy of science.

Most of the replies stressed the importance of the subject in university curricula. In the case of science students such courses were seen as a means of integrating knowledge of the physical world with the humanities, and as a counter to specialisation. In the case of arts students it was thought that courses of this sort enabled them to obtain a general scientific outlook, and to become conversant with the influence of science on philosophy and culture. It was also suggested that the history of science should form part of post-graduate courses of study in education, philosophy and history. For philosophers history of science was seen to be particularly important as science and philosophy have been so closely inter-related in the past.

Melbourne suggested that there are two distinct functions of a course in the history and philosophy of science, (1) as a discipline in its own right and (2) as allowing an opportunity for inter-faculty communication—bridging the gap between arts and sciences. It also made four further points which are worth quoting since they have some bearing on the development of the subject.

(1) The need for junior courses to be specially designed according to the background of the audience.

(2) The members of the department were all convinced of the benefit accruing from their position as an autonomous department. They felt that they would be at a serious disadvantage if they were a sub-section of History, or Philosophy, or any one of the science departments. They urged the appointment of a professorial head of the department, who would be able to look after the interests of the subject in the Senate.

(3) They were convinced of the need for a group of people with different academic backgrounds to handle the different special sciences. It was erroneous to believe that one person can deal with all branches of the history and philosophy of science.

(4) They were also convinced of the importance of teaching the combination of history and philosophy of science rather than teaching these two disciplines separately.

Another point brought out by our inquiry is that interest in the subject seems partly dependent on the size of the university, its date of foundation, and the priority it gives to cultural questions *vis-à-vis* bread and butter ones in its curricula. It may be that a university has to achieve a certain size and prestige before it can afford the presumed luxury of courses in the history and philosophy of science.

As far as future developments are concerned this survey does bring out the need for establishing a number of new posts in both teaching and research. The newer universities might be encouraged to found chairs in the history and philosophy of science. In those universities where such a course might be impracticable it might perhaps be possible to transform certain existing chairs to ones in this subject. Some thought might also be given to the possibility of instituting an Honours course at one or other of the British universities, as has already been done at Melbourne. With the increasing impact of science and technology on everyday life such courses should become progressively more important.

There is certainly some ground for Melbourne's demand that independent departments should be set up in universities. Otherwise the history and philosophy of science seems fated to remain an ugly duckling in the department which acts as foster parent. The failure so far of the subject to achieve an independent status in most universities, may perhaps arise from the fact that it overlaps a number of different disciplines. The 'establishment' in these disciplines may

feel that the development of the history and philosophy of science could only be at the expense of their own subject.

A very valuable field to which teaching in the history and philosophy of science may be extended is the sixth forms of schools. Some schools have already started to give instruction. The Oxford and Cambridge Joint Board and the Cambridge Board now set G.C.E. papers in the history and philosophy of science. We may eventually find ourselves with a generation of young men and women with some school training in this subject which they would be able to continue on a university level.

There would also seem to be important teaching possibilities in the field of extra-mural studies. If the present rate of scientific progress continues, the enlightened man in the street may be expected to show a greater interest in science as a cultural phenomenon. Departments of education will also need to give some instruction to those who are going to teach the history and philosophy of science in schools and elsewhere.

In the case of arts students more could be done in the teaching of the philosophy of the social sciences. Arts students would have much less difficulty in understanding the philosophical foundations of the social sciences than that of the natural sciences. In this way they might attain some appreciation of scientific methodology without needing to have specialised knowledge of, say, physics and chemistry. Although the social sciences have only emerged comparatively recently, the philosophical problems they raise are often of extreme importance.

2 *Summary of Results*

Great Britain

What emerges from the Leeds enquiry is that there were only four universities where the subject may be said to be firmly established. The oldest department is that of the History and Philosophy of Science at University College, London, established nearly thirty years ago. It is, however, largely a research department with the main emphasis on the history of science. Another established London department is that of Philosophy, Logic, and Scientific Method at the London School of Economics. Philosophy of science is studied there, both at an undergraduate and research level. Philosophical problems concerning the history of science are also dealt with. The Oxford and Cambridge posts in the subject are more recent and are post-Second World War creations. The interest of most of the other British

universities, with the exception of Aberdeen, seems only to be of fairly recent date.

In the modern universities, a number of lecturing posts have been established in both the philosophy and history of science, as well as in these subjects separately. Extra-mural posts in each subject have also been established. At other universities the history of science is taught by scientists, philosophers or education department staffs. For some years there has been a Reader in the History and Philosophy of Science at Aberdeen, who has done a great deal to stimulate interest in the subject both in his own University and other Scottish universities. A number of universities offer lectures and seminars to post-graduate, research, or education students.

As far as degrees, diplomas, and certificates are concerned, there is as yet in this country no Honours degree in the subject. The Cambridge certificate in the History and Philosophy of Science can be taken in lieu of Part II of a Tripos, as Part I normally qualifies for the B.A. There has been some discussion in Cambridge as to the institution of an Arts degree in the theory and methods of science. There is at Oxford a diploma in the History and Philosophy of Science. One can also take research degrees, Ph.D. at Cambridge, D.Phil. at Oxford, M.Sc. and Ph.D., at London, and M.Sc. at Leicester. It has sometimes been argued that the subject is one better fitted to be taken at a post-graduate level than an undergraduate level, that some maturity of mind and background in other subjects is necessary before it can properly be studied. Though this may perhaps apply to some extent to the philosophy of science, it is doubtful whether it is true of the history of science.

Australia and New Zealand

The Australian universities seem to be well ahead in this subject. It is not clear whether this is due to Melbourne's example or to other reasons. Certainly some of them are large in size and have a large intake of students. This may have led them to introduce the history and philosophy of science in an attempt to solve problems relating to the integration of the diverse disciplines in the curriculum. Melbourne has an autonomous department with a staff of six and two part-time tutors. At the Australian National University and Canberra University College appointments in the history of science are contemplated. Queensland and Sydney each have a senior lecturer in the history of science, though the Sydney post was unfilled when the questionnaire was returned. (*contd. on p. 210*)

GREAT BRITAIN — Courses in the History and Philosophy of

UNIVERSITY	TYPES OF COURSE AND TREATMENT
Aberdeen	*Philosophy of Science.* One lecture per week for 4th year scientists, 9 lectures for 5th year medicals. Evening seminar for staff and post-graduates. *History of Science.* Weekly course for 3rd year Science. 3-lecture introductory course for 1st year Science. 5-lecture course for 1st year medicals. 5-lecture course for 3rd year Chemistry. Lecture seminars in Dept. of Modern History.
Bristol	General B.Sc. 3rd year: 5 lectures on Philosophy of science; 31 Lectures on development of various sciences.
Cambridge	1. Certificate (Post-Part I) in History and Philosophy of Science. Covers one year full-time instruction in History and Philosophy of Science. 5 papers set. 2. Optional half subject in Natural Science Tripos Part I. 3 lectures a week and discussion classes. University investigating possibility of a combined Arts and Science course.
Durham	
Exeter	Occasional lectures for undergraduates (and potential teachers). Regular (1 per week) in Education Dept.: outline of History of Science to present day. Course in Scientific Method for science teachers.
Hull	History of Science for post-graduate students, mostly scientists and mathematicians. 19 lectures. Philosophy of Science for undergraduates.
Leeds	Courses on History of Science: B.A. and B.Sc. mixed class; 1st year B.Sc. General and Special subsidiary 2nd year.
Leicester	Optional course for all General Arts and General Science students, and some Special Science students. 3 lectures per week: survey from beginnings until technical matter becomes too difficult for a mixed audience. Course for Classical Philosophy students. History of Science in part time 3-year certificate course for experienced science teachers. Adult education courses.
Liverpool	Courses in History and Philosophy of Science are under discussion, particularly for B.A. General.

SCIENCE IN BRITISH AND COMMONWEALTH UNIVERSITIES

POSTS ESTABLISHED AND EXPECTED APPOINTMENTS	TAUGHT BY	BALANCE BETWEEN HIST. AND PHIL. OF SCIENCE	RESEARCH
Reader in History and Philosophy of Science.	Staff of Department.	Largely History of Science.	—
—	Science and Philosophy Lecturers.	Largely History of Science.	—
2 Lecturers and 1 Assistant Lecturer.	—	Both.	Ph.D.
Post in Philosophy of Science established	—	—	—
—	Lecturer in Education Dept.	Largely History of Science.	—
—	Education and Philosophy Lecturers.	Both.	—
Post in History and Philosophy of Science.	—	History of Science.	—
Post in History of Science. Post in Philosophy of Science in Philosophy Dept.	—	Largely History of Science.	Two-year M.Sc. course in the History of Science.
—	—	—	—

GREAT BRITAIN

UNIVERSITY	TYPES OF COURSE AND TREATMENT
London School of Economics	Dept. of Philosophy, Logic and Scientific Method. Scientific Method for Hons. Economics. Philosophy of the Natural and Social Sciences for Joint Degree in Philosophy and Economics.
London University College	Dept. of the History and Philosophy of Science. Courses for M.Sc. and Ph.D.: History of Science to nineteenth century. Philosophy of Science. M.Sc. Exam.: Part I. Three papers in History of Science. One in Philosophy of Science. Part II. Dissertation. Undergraduate teaching to history specialists.
Manchester	Course on History of Science, mainly to physics students. Science graduate seminar on Philosophy of Science. Examinable arts course in Philosophy of Science.
Newcastle	Philosophy and some History of Science for Dip.Ed.
North Staffs.	History of Science for 1st year students in science and some non-science students. Philosophy of Science for 1st year students. Special subject course for final year Philosophy students.
Nottingham	General lectures for all students contain one or two per year on History of Science. Part of Extra-Mural Studies.
Oxford	Optional supplementary subject in Honours School of Natural Science. Possible to offer it in satisfaction of the requirements of Part II, 4th year Chemistry. B.Phil. optional paper on ' Chosen period of Scientific Thought '. Post-graduate Diploma. Lectures to undergraduates on science in the 19th and 20th centuries by group of science lecturers. Subject mainly developed at 4th year and post-graduate levels. Proposed discussion for the introduction of a special subject on the History of Science in the Honours School of Modern History.
Reading	Some science departments give short historical introductions to their subjects.
Sheffield	Examinable course for 2nd year Science and 4th year education : General History of Science ; individual history of particular sciences ; dissertation for all students.
Swansea	

POSTS ESTABLISHED AND EXPECTED APPOINTMENTS	TAUGHT BY	BALANCE BETWEEN HIST. AND PHIL. OF SCIENCE	RESEARCH
Professor. 2 Readers. 2 Lecturers.	Staff of Dept.	Largely Philosophy of Science.	Post-Graduate Research in Philosophy of Science.
Professor. 2 Readers. 2 Lecturers. Part-time Lecturers.	Staff of Dept.	Largely History of Science.	Department mainly a research one. All students part-time, except students from abroad.
—	Philosophy Lecturer.	History and Philosophy of Science.	—
—	—	—	—
—	Lecturers in Physics, Chemistry, Philosophy.	Both.	—
Post in Extra-Mural Studies	—	History of Science.	—
Senior Lecturer in History of Science. Reader in Philosophy of Science. Reader in Philosophy of Mathematics.	—	Both.	D.Phil.
—	Members of Dept. concerned.	History of Science.	—
—	Scientists, Extra-Mural Lecturer, 2 Lecturers in Education (both scientists).	History of Science.	Yes.
Post in Philosophy of Science established.	—	—	—

IRELAND

UNIVERSITY	TYPES OF COURSE AND TREATMENT
Queen's University, Belfast	Courses for Philosophy Honours Students: History of of Dynamics; theories of space and time from Descartes to Einstein. Course to 3rd year Arts students in 1960–1.
Trinity College, Dublin	Optional course for Hons. B.A. degree in Philosophy on the philosophy of the physical sciences. Arts course for professional students: option in History and Philosophy of Science at simple level.

AUSTRALIA

Canberra University College	
Melbourne	Arts Pass 1st year. Arts Hons. 4-year course. Architecture 1st year. B.Sc. (B. course) 2nd or 3rd year students qualifying for a science degree for teachers. B.Sc. Hons. 4th yr. (Chemistry). B.Sc. Pass and Hons. 3rd yr. (Physics). Premedical voluntary course, 20 lectures. Medicine Div. 1A, 12-hr. course on Scientific Method.
Australian National University	
New England	Was introduced as part of Philosophy course for Arts and Science students but received no support from Science—confined to logical problems.
New South Wales	Sometimes included in Philosophy curriculum in Humanities and Social Sciences. Forms part of treatment of Scientific Method. Expected to be a required course in new Arts degree.
Queensland	2nd year Science students—1 lecture, 2 seminars per week.
Sydney	Compulsory additional course for 3rd year Science students. All Science departments are required to help students to become familiar with the history of their subject.

POSTS ESTABLISHED AND EXPECTED APPOINTMENTS	TAUGHT BY	BALANCE BETWEEN HIST. AND PHIL. OF SCIENCE	RESEARCH
Lecturer in the History and Philosophy of Science (1958).	—	Both.	—
—	—	—	—
Appointment expected.	Will be taught by someone qualified in Science and Philosophy.	History of Science with scientific theory and method.	—
Senior Lecturer in charge. 5 Lecturers and 2 part-time Tutors. Autonomous department in Faculty of Arts	Members of the Department of the History and Philosophy of Science.	Both.	M.A. and Ph.D. Research seminars.
Appointment expected.	—	Appointment will be for both.	—
—	—	—	—
—	Philosophy Lecturers.	Both.	Possibility will be considered.
Senior Lecturer.	—	History of Science.	Hoped to extend course to include this.
Senior Lectureship at present advertised.	—	History of Science.	—

CANADA

UNIVERSITY	TYPES OF COURSE AND TREATMENT
Laval, Quebec	History of Philosophy and Philosophy of Science.
McGill	Course offered in History of Science by member of Philosophy Dept. Course in History of Medicine.
McMaster	Half-year course in Philosophy of Science open to anyone who has previously taken philosophy. 3 hrs. per week. New course on the fundamentals of Physical Science for non-scientists.
Montreal	Compulsory courses for Science and Engineering students. Course for extension students and B.Sc. General (optional). Development of the pattern of thought in Western Civilisation.
Mount Allison, Sackville, N.B.	3rd and 4th year course for Arts, Science, Engineering (compulsory for latter). One lecture per week, but mainly reading course.
Ottawa	History of Medicine. Basic course in Philosophy of Science as part of Philosophy course for Science students. Historical survey in each science.
Saint Mary's Halifax	Elective course giving outline of scientific progress and tracing its influence on literature and political thought.
Saskatchewan	Compulsory Philosophy of Science for Honours Philosophy, available for all students with science background.
Sir George Williams College, Montreal	Half-course in History of Science : development of science to nineteenth century. Full course in Philosophy of Science.
Western Ontario	History of each science subject, especially history of medical science. 3rd and 4th year Science Honours undergraduates. Post-graduates.
Assumption University of Windsor, Ontario	2nd year General Arts, Science, Commerce attend Philosophy of Science, in which some history is included.

POSTS ESTABLISHED AND EXPECTED APPOINTMENTS	TAUGHT BY	BALANCE BETWEEN HIST. AND PHIL. OF SCIENCE	RESEARCH
Appointment expected.	—	—	—
History of Medicine.	Philosophy Lecturer. Professor of History of Medicine.	—	—
—	Scientists and Philosophers.	Largely Philosophy of Science.	—
—	Teachers competent to fill the needs of the curriculum.	History of Science.	—
—	Dean of Science Faculty (mathematician).	History of Science.	—
—	Scientists and Philosophers.	Both.	—
—	—	History of Science.	—
—	Philosophy Lecturers.	Philosophy of Science.	—
—	Science and Philosophy Lecturers.	Philosophy of Science.	—
Sessional Lecturer in Medical History.	Members of Science Faculty.	History of Science.	Research graduates must have seminar course in History of Science.
Professor of Physics and Philosophy of Science.	Professor.	Philosophy of Science.	One course in problems of history of modern thought for Philosophy graduates.

HONG KONG

UNIVERSITY	TYPES OF COURSE AND TREATMENT
Hong Kong	History of Science (optional) for Dip. Ed.; 4 lectures per year. Approved course in History of Science for 1st year B.A., at present not taught because no suitable staff. It is hoped to implement the course in the future.

INDIA

Annamalai	1st and 2nd year Humanities students : development of science from beginnings to present.
Delhi	2nd year B.Sc. Pass and Honours students : development of Physical and Biological sciences.
Karnatak	Optional course for all 3 years of integrated B.A. and B.Sc.: Histories of various sciences.
Maharajah Sayajirao of Baroda	Compulsory course in preparatory 1-year Arts and Commerce : outline of development of science through the ages.
Poona	Scheme of extension lectures has been adopted. Philosophy of Science for M.A. students.
Punjab	To be introduced at undergraduate and graduate levels as part of regular courses.
Rajasthan	Part of Logic and Scientific Method.
Visva-Bharati	History of Science compulsory for undergraduates. Philosophy of Science optional for post-graduates.

POSTS ESTABLISHED AND EXPECTED APPOINTMENTS	TAUGHT BY	BALANCE BETWEEN HIST. AND PHIL. OF SCIENCE	RESEARCH
Post contemplated in History Dept.	—	History of Science.	—
—	Chemistry Lecturer.	Largely History of Science.	—
Post established but not filled.	Science Lecturers.	History of Science.	—
Appointment expected.	—	Largely History of Science.	—
—	Members of Faculty of Science.	History of Science.	—
—	—	Both.	—
—	Science Lecturers.	History of Science.	—
Appointment expected.	—	—	—
Appointment expected.	Science Lecturers.	Largely History of Science.	—

NEW ZEALAND

UNIVERSITY	TYPES OF COURSE AND TREATMENT
Auckland	Philosophy course includes some reference to foundations of modern science.
Canterbury	Subject dealt with independently according to views of departmental heads.

PAKISTAN

Sind, Hyderabad	History and Philosophy of Zoology for post-graduates: Principles of General Biology, deals with development of concepts underlying evolution and genetics. B.Sc. Hons. courses in History of Chemistry and History and Philosophy of Physics.

SOUTH AFRICA

Cape Town	Honours and post-graduate Science students: History of Science to seventeenth century; discussion of aspects of modern science.
Orange Free State	2nd and 3rd year Science students: outline of Cosmology, Epistemology, Scientific Method and Theory.
Potchefstroom	Maths., Physics and Biology students 3rd year: brief survey showing principles underlying scientific theory and research.
Pretoria	History and Philosophy of each science as part of basic course.
Stellenbosch	History of scientific achievements dealt with in appropriate places in science courses.
Witwatersrand	History of Science started in 1959. 3rd and 4th year B.Sc.; 8 lectures in each of two of students' major subjects. Philosophy of Science for B.Sc. and B.Sc. Honours 3rd year; 2 lectures per week for half-year.

POSTS ESTABLISHED AND EXPECTED APPOINTMENTS	TAUGHT BY	BALANCE BETWEEN HIST. AND PHIL. OF SCIENCE	RESEARCH
—	—	—	—
—	—	—	—

—	Scientists.	Largely History of Science.	—

—	Scientists.	History of Science.	Course for post-graduate science students.
—	Professors of Mathematics and Botany.	History of Science.	—
—	Scientists.	History of Science.	—
Appointment expected.	Scientists.	Both.	—
—	Scientists.	History of Science.	—
Science Faculty has recommended that a post be established.	Scientists.	Largely History of Science.	—

WEST INDIES, RHODESIA and NYASALAND, NIGERIA

UNIVERSITY	TYPES OF COURSE AND TREATMENT
University of West Indies	In the Education department prospective science teachers have questions in the Philosophy of Science raised with them.
University College of Rhodesia and Nyasaland	Some use of the historical approach in the teaching of general courses in Botany, Zoology, Chemistry and Physics.
Ibadan, Nigeria	2 or 3 lectures on History of Science given occasionally in some science departments to Part II Special Honours Students.

(*Contd. from p. 197*)

Apart from its Honours School and research students working for post-graduate degrees, the numbers of which are as yet small, Melbourne seems to act largely as a service department. One feels that the Faculty of Arts at Melbourne must be a very enlightened one, in the first place for setting up an autonomous department, and secondly for agreeing to the introduction of an Honours degree in the subject. It seems clear from the Melbourne experiment that if such a department was started at other universities there would be no shortage of work which could be found for it.

The New Zealand universities do not report very much in the way of activity in this field. They presumably have smaller staffs and a smaller number of students to deal with, and the problem of inter-disciplinary integration is probably not so acute.

Canada

Even among the smaller Canadian universities there seems to be some interest in both the philosophy and the history of science. Philosophy of science courses seem to be given as frequently as history of science courses. In some of the university medical schools the history of medicine is offered as a course. There do not, however, seem to be any post-graduate or graduate degrees or diplomas in the history and philosophy of science. Assumption University has a Professor of Physics and the Philosophy of Science. At McGill

POSTS ESTABLISHED AND EXPECTED APPOINTMENTS	TAUGHT BY	BALANCE BETWEEN HIST. AND PHIL. OF SCIENCE	RESEARCH
—	—	—	—
—	—	—	—
—	—	—	—

there is an established post in the History of Medicine. At the University of Western Ontario there is a sessional lecturer in the History of Medical Sciences.

India and Pakistan

Indian and Pakistani universities use the history and philosophy of science mainly for giving science students a historical background and for broadening the interests of arts students. The subject is mainly taught by science and philosophy lecturers. One university, Karnatak, is contemplating a teaching post in the history of science. At Delhi there is a lectureship in the subject, but this remains unfilled. Most of the replies emphasise the need for some historical study of science.

West Indies, Ibadan, Rhodesia and Nyasaland, and Hong Kong

Out of these four widely scattered universities, Hong Kong seems to be the only one which is contemplating giving a separate course in the history of science. Perhaps this is due to its being an older foundation, and, as it were, the meeting point of two great civilisations—Chinese and European.

The University,
Manchester 13

CONDITION, CAUSE, FREE WILL, AND THE DIRECTION OF TIME

H. C. Plaut

There have been repeated attempts to consider the direction of time as determined and—as it were—defined by the concept of causality. This idea can already be found in Kant's *Critique of Pure Reason*, and more recently Ayer[1] and Schrödinger[2] have made use of it. Good[3] also discusses it. Ayer says in this connection: 'Our notion of causality is derived from the experience of human action.' But he does not follow up the idea any further. In his book *Space, Time and Gravitation*, written as early as 1920, Eddington expresses fundamental ideas, which connect free will, causality, and the direction of time as opposed to determinism. He does not, however, deal here with the closely related concept of condition. Eddington's main idea, which I believe to be a fundamental truth, appears to have been overlooked by many later writers, and there has been a certain amount of confusion. In this paper, an attempt will be made to clarify the whole problem, including the concept of condition. I shall try to investigate to what extent the concepts of condition and causation are based on indeterminism, and to ascertain what follows logically from the assumption that personal free will—which I believe to be the only tangible macroscopic manifestation of an indeterministic world—is not an illusion. We shall find that each of these concepts is used in two widely differing ways, one of which refers generally to qualities of classes. It can be defined by symbolic logics and holds in a deterministic as well as in an indeterministic world. The other refers to individual persons and circumstances and does not make sense in a perfectly determined world. It can be defined only by words which already presuppose indeterminism, or by pointing at occurrences actually experienced. We shall further find that from the personal experience of free will—if it is not an illusion—there follows inevitably the existence of physical forces in the sense of an 'impressed force' and the dynamic character of the

[1] A. J. Ayer, *The Problem of Knowledge*, London, 1956, p. 192
[2] E. Schrödinger, *Mind and Matter*, Cambridge, 1958, p. 79
[3] I. J. Good, this *Journal*, 1959, **9**, 307

inanimate world. Lastly, we shall consider the direction of time and shall find that its definition from either of the two concepts of causality involves a logical circle, but that criteria for it can be found in so-called reversible processes in very much the same way as in irreversible processes. We shall assume, for the time being, that man is free to decide and act. I shall return to this question later.

1 *Causality*

Before we start treating the somewhat complex problem of causality, I shall give an explanation of some of the terms used: The concept ' determinism ' can be illustrated by the metaphor of chains of events. There can be four kinds of such ' chains ': firstly chains that reach from infinite past to infinite future; secondly chains that start at a definite time, but reach into infinite future; thirdly chains that come from infinite past, but end at a definite time; and lastly chains that are finite in both directions. A single unconnected event can be considered a special case of a finite chain. As the connections created by the laws of Nature are so many-fold and far-reaching that no event is known that has no consequences at all, we can omit the last two categories of chains for the time being. We call a world perfectly deterministic in which only chains of the first kind exist, i.e. such that are infinite in both directions. This does not exclude that they branch off and interconnect. In a partially deterministic world, there are chains of both the first and the second kind. Chains can be started by free will; this is what our axiom postulates. For the present, it does not matter whether it is my own free will or the free will of any other living being. If a chain is started, but not by free will, we attribute its starting to absolute chance. This definition of absolute chance may comprise events like the emission of a particle in radioactive decay or the will of a higher being, or—if only living beings can start a chain—it may define an empty class.

We shall now replace the metaphor of chains by definitions based on the concepts of classical macro-physics.[1] We consider events to be determined if they can be foretold from the condition of the world at a prior instant (boundary conditions) and from the laws of nature which generally take the form of differential equations like those used

[1] A more general definition which, if so desired, can be substituted for ours, was given by Northrop (Introduction to W. Heisenberg, *Physics and Philosophy*, London 1958, p. 21).

in Newton's and Maxwell's theories. Events which cannot be foretold in this way, we shall consider as undetermined.

The concept of causation as opposed to that of condition presupposes that the cause precedes the effect or coincides with it, and that both events occur at least once. This concept can be used in three ways :

(1) *Kinematic Causation:* An event A is said to cause an event B, if A precedes or is coincident with B, and B never occurs otherwise. In this definition it is supposed that there is one class C_A comprising like events of a kind A, and another class C_B comprising like events of a kind B, and that an element of the latter class is always preceded by one of the former. An example of such causation from everyday life is the primitive observation of lightning and thunder. Kinematic causality can be ascertained by comparing different chains or different regions of the same chain. We call this kind of causation kinematic (in contrast to dynamic causation). It could also be found in a film consisting of mere patches of colour without inner connection, e.g. if red were always preceded by blue.

(2) *Initiating Causation:* If we apply the definition given above not to classes of events, but to individual events, we are led to a concept that we shall call initiating causation. Here every event is considered to be the cause of all the succeeding links in the chain. With endless chains, this definition is trivial. With starting chains, however, it puts the first event in a special place, and the others may be characterised by their position in the chain, e.g. their distance from the start. It is irrelevant for our purpose whether we consider free will or absolute chance as the first event, or whether we start counting with the first actual event.

(3) *Dynamic Causation:* This is the popular meaning of causation. It assumes that events have the power to create or to influence later ones. We call it dynamic, because historically it has evidently developed from the application of muscular force. The investigation as to what extent this somewhat vague concept is based on experienced facts, will form an essential part of our further considerations.

The following four examples will further illustrate the concept of causality. Example (*a*) represents kinetic causality, (*b*) is ambiguous, (*c*) illustrates initiating and (*d*) dynamic causality.

(*a*) ' Sunshine causes the snow to melt.' This sentence would hold in a perfectly deterministic as well as in an indeterministic world. In

so far as it is confirmed by unreasoned observation, it can be based on kinematic causality.

(*b*) ' The mountaineer was killed by a fall of stones.'—' A stone which hit his head, caused the mountaineer's death.' We can look at this event in two ways: if we assume complete determinism, this example represents an irreversible predetermined course of events. If however we admit the existence of chance (in its absolute sense), we can consider this sequence of events as constituting a newly starting chain. But here we encounter the difficulty that every event is dependent on a large number of circumstances. The choice appears quite arbitrary which of these circumstances is to be considered the cause of a certain event. In the case of the mountaineer, any of the following may be regarded as the cause of his death: the stone that actually hit him; any obstacle in the path of this stone that deflected it towards the man; any other stone which started the last one rolling, beginning with the one that started the avalanche; each step that brought the man nearer the place of his death. The solution of this problem is simple: the concept of causality, as it is seen in this example, is based on the assumption of a ' ceteris paribus '; i.e. we assume that, all other circumstances being equal, the event we call the cause might or might not have occurred. But there is something artificial in this limitation: when can we actually assume that an individual physical event might or might not have occurred, quite independently of the state of the world in other respects? And how can we point to any one of the circumstances which might be regarded as causes, and say: ' This one was due to absolute chance'?

(*c*) ' By offending him, I made him my enemy.' Here I was free to decide whether to utter the offending word or not. The start of the chain of events is quite clearly defined: it started at the place and at the time when I uttered the offending word. The ' ceteris paribus ' can also be clearly defined: it comprises all those circumstances which are in no way either directly or indirectly influenced by my decision. This is of paramount importance. Only in a case where a chain of events is started by an act of will, can its start be witnessed.

(*d*) ' By winding up the chain of a winch, I cause the load to rise.' If I do not make the appropriate movements, the load will not rise. If the chain is too weak, it will break, and the load will not rise either.

These examples lead to the following conclusions:

Kinematic causality is applicable in general cases only. It presupposes the existence of classes or sets, of which the individual cases

are elements. This is the sense in which Northrop[1] uses the word ' causation ', but not its popular usage. The application of kinematic causation is justified by induction. That it is paradoxical to apply this concept in some individual cases, can be seen from the following sentence: ' My proposal to Miss A. on that day was the cause of our marriage.' It cannot be replaced by the sentence: ' Each time I proposed to her on that day, we married.' Kinematic causation applies in a deterministic and in an indeterministic world.

Initiating and dynamic causation apply in an indeterministic world only. They refer primarily to individual cases, because the beginning of a chain is an individual occurrence. A number of similar special cases, in which initiating or dynamic causation apply, can be united in a class, and for such classes kinematic causation can be used. A sentence which illustrates this procedure, and in which the word ' cause ' is used at once for kinematic and initiating causation, is the following: ' Contempt for the lower classes is the cause of revolutions.' This sentence states the starting of millions of chains on the psychological level, and at the same time uses kinematic causation on the historical level, whole nations being the single elements.

The popular concept of causation does not stop at initiating causation, but proceeds to dynamic causation in applications to the physical world. There is the following interesting line of thought: as in the chain of events started by the act of free will only the first link is simultaneous with this act of will or follows it immediately, the successive links further down the chain can hardly be supposed to be created *directly* by this will, and therefore each link in this chain must be supposed to create the following one. Hence events have the same power as free will to create succeeding events. Similar conclusions can also be drawn if we suppose that chains can be started by absolute chance.

These or similar considerations have doubtless led to the formation of the concept. But can it truly be said that dynamic causation really exists? Three objections can be raised: (1) The chain we have been talking about is only a metaphor. The real world is three-dimensional and continuous with innumerable interconnections. (2) The metaphor supposes that the effect is directional, from each link to the next one. But the law of action and reaction tells us that the effect of objects

[1] A more general definition which, if so desired, can be substituted for ours, was given by Northrop (Introduction to W. Heisenberg, *Physics and Philosophy*, London 1958, p. 21).

on each other is mutual and symmetrical. (3) In the metaphor, the effect has a direction in time, but in the physical world, in the case of reversible reactions, time can be reversed.

We answer these objections by referring to example (*d*). We replace the ' chain of events ' by a real chain, consisting of iron links with a definite mass and modulus of elasticity, and assume that the strains produced are reversible (except in the case of breakage). We furthermore assume that the winch is actuated by a handle, and that the weight is fastened to a hook. If we turn the handle, the stretching and movement of the links passes along the chain in a finite time, determined by their mass and modulus of elasticity, and the weight is lifted. This occurs every time we turn the handle, provided the connection holds; never if we do not turn the handle, or if a link is too weak. We conclude by induction that this will always be the case.

The second objection is not valid: there is mutual reaction between the links; but nevertheless, the effect passes along in one direction down the chain. Reactions pass along the chain in the opposite direction and are felt in our hand. Both action and reaction are directed in time. Objection (2) therefore cannot hold.

As to the reversibility of time in the differential equations of mechanics, the position is as follows: this reversibility is a property of our mathematical apparatus. Though extremely interesting in itself, it is only made possible by the fact that the formula does not contain the whole information conveyed by the experiment; it leaves out the actual direction of time. In applying the formula, we put this part of our information back by coupling in the boundary conditions the appropriate direction of velocities with the progressive direction of time.

In example (*d*), the winch need not be actuated by a human hand, but could be driven by a windmill, which starts working when the wind becomes sufficiently strong. The movement passing down the chain and the lifting of the load would be the same. This forces us to the conclusion that dynamic causation also applies in the absence of free will or absolute chance. In human life, there are innumerable cases where our free will initiates a course of events similar to that exemplified by the winch. The concepts of dynamic causation and of physical force are based on the whole of this experience, which creates an enormous number of associations that come into play when these concepts are applied. Everyone knows how to apply them. Not

much more can be said. But I do not believe that more can be said about any semi-empirical concept than that is it based on an enormous number of experiments, and that everyone can apply it. If free will is no illusion, neither is the dynamic character of the world. It cannot be recognised by passive observation. It is revealed and indubitably confirmed by innumerable experiments.

2 *Condition*

The concepts of condition and causation are closely related. Both refer to the occurrence or non-occurrence of two events in connection with each other, and the same facts can often be expressed by conditional as well as by causal sentences: 'If the sun shines on the snow, it will melt.' 'If I turn the handle of the winch, the load will be lifted.' (Corresponding to examples (*a*) and (*d*) respectively.)

There is here the same fundamental division as in the case of causality between conditional sentences referring to general classes and those referring to individual events. In the first case, the word 'if' can be replaced by 'whenever'. Conditional sentences of this kind, which correspond to kinematic causality, make sense in a deterministic as well as in an indeterministic world. Conditional sentences of the second kind correspond to initiating causation and make sense only in an indeterministic world.

The concept of condition differs from that of causality in that it does not presuppose a time-order between antecedent and consequent. If A is a sufficient condition of B, B is a necessary condition of A, and vice versa.[1] Furthermore, the concept of condition assumes that the antecedent may possibly not occur, or may at least be imagined not to occur. It always contains an element of uncertainty, which can arise in an indeterministic world from indeterminacy or from lack of knowledge, in a deterministic world from lack of knowledge only.

3 *Logical Structure of Condition and Causation*

In logical terms, our results so far can be summarised as follows:

Necessary condition: non-A entails non-B; class non-A is not empty.

Sufficient condition: A entails B; class non-A is not empty.

[1] Ayer, op. cit. p. 93

Causation: non-A entails non-B[1]; class B is not empty; B never precedes A in time.

These three statements provide structural relations between events. They are completely general. They may be applied either to a deterministic or to an indeterministic world. We have now to discuss what instances can be substituted for A and B, firstly in an indeterministic, secondly in a deterministic world.

In an indeterministic world, these instances can be actual events—imagined events—possible events. In a deterministic world, there are no possible events that are not actual events, i.e. they always are or have been or will be realised.

Kinematic causation and the corresponding concept of condition are based on the characteristics of classes and on induction. They therefore always require a plurality of instances, which are considered as elements of classes. They refer primarily to classes and can be applied to individual events only by specialisation. They make sense both in an indeterministic and in a deterministic world.

Initiating and dynamic causation and the corresponding form of condition refer to the starting or non-starting of ' chains '. They therefore refer primarily to individual events and can be applied to classes only by generalisation. These concepts refer to possible events, which may or may not be realised, and do not make sense in a deterministic world. The upshot is that a purely deterministic world cannot include initiating and dynamic causation, because these refer to the choice between possible instances, upon the outcome of which depends what is actually realised.

4 *Direction of Time*

It follows from our analysis of kinematic and initiating causality that neither can be used to define the direction of time, if ' definition ' is understood in its strictly logical sense, because in either case a logical circle would arise. In the definition of kinematic causation, it was stipulated that the cause precedes the effect. Thus the definition presupposes the temporal relation. In the case of initiating causation the definition is based on the starting of chains. Here too, ' starting ' presupposes the direction of time.

[1] This refers to ' *the* cause '; for ' *a* cause ', A is replaced by a disjunction, one of whose terms is A. For definitions of ' *a* cause ', based on probability, see Good, op. cit.

Suppose however we abandon the strictly logical attitude, i.e. of defining in general the direction of time, and instead fall back on seeking a criterion that indicates the direction of time in individual cases. To illustrate, consider a sequence of photos of a garden containing a growing plant at various stages of its life. Since we know that growth is irreversible, the time-direction must be from small to large. Hence we can know the correct time-order of the photos. A reversible process might be used just as well: if the sequence of photos contained a scaffolding being erected or dismantled, we could still know the time-order if we were given the initial conditions, i.e. if we knew which picture was taken first. Other similar procedures, whether based on causality or not, can be used as criteria for the direction of time, but only against the background of our knowledge of the meaning of ' before ' and ' after '. None of them enables us to *define* the direction of time, because all of them contain the notion of it already. This is one of the chief points I wish to make.

I might mention that Einstein makes use of a light-signal to define ' before ' and ' after ' at a distant point. This is perfectly legitimate: he supposes that the meaning of ' before ' and ' after ' is known in the immediate proximity of the observer, and he uses it to *define* these concepts *at distant points*. Some writers appear to consider this definition as a proof that the direction of time is determined by causation. Their view that the use of a beam of light brings in causation, may be justified. But they overlook the fact that ' before—after—simultaneous ' at a distant point are logically not identical with the experienced features ' before—after—simultaneous ' in our sensual perception.

Summary

(1) There are two quite different ways in which each of the two concepts ' causation ' and ' condition ' are usually applied. The first is based on properties of classes and derives its justification from experience by induction. It is generally used in science. I have applied the term ' kinematic causality ' to this form of causat on. This as well as the corresponding form of condition make sense in an indeterministic as well as in a perfectly deterministic world. Secondly, there is a form of condition and causation which presupposes that chains of events can be started by chance or free will, and that they might or might not occur. I have applied the terms ' initiating ' and

'dynamic causality' to this kind of causation. These as well as the corresponding forms of condition are those normally used in everyday language. In a perfectly deterministic world, they do not make sense. Any other forms of causality and condition have not been dealt with in this paper.

(2) Free will—if it is not an illusion—is the most tangible macroscopic manifestation of indeterminacy, because it can be witnessed within narrow limits of space and time.

(3) From the assumption of personal free will follow inevitably the idea of an impressed force and the dynamic character of the inanimate physical world.

(4) A definition of the direction of time can be based neither on kinematic nor on initiating causality. Both would involve a logical circle.

I do not claim to have disproved determinism. But proving it by kinematic causation can only be an extrapolation. A proof based on initiating or dynamic causation, which presuppose an indeterministic world, could only take the form of a *reductio ad absurdum*. If determinism is true, we should have to discard the concept of dynamic causation.

I have said nothing about atomic statistics and have touched epistemological problems only in the last paragraph of Section 1. Much remains to be said on these points, but after all some limits had to be set to the scope of this paper.

45 Banstead Road South,
Sutton, Surrey

DISCUSSION

Physics and Chemistry: Comments on Caldin's View of Chemistry

I have read with interest E. F. Caldin's article 'Theories and the Development of Chemistry' in the *Journal* for November, 1959. As I understand it, Caldin wishes to point out that there is some contrast between the analytical or critical assessment of scientific activity as involving attempts to *refute* theories, and how in fact the practising scientist makes use of theories in his work. Caldin draws his examples mainly from the field of chemistry, and more particularly, physical chemistry, although at times he also makes some mention of fundamental theory in physics. It may be of interest to mention that quite urgent problems of this kind sometimes appear to arise when physicists and chemists are themselves involved together in scientific discussion.

Caldin says: 'The point is that chemists are interested in finding out what goes on in nature, as well as in explaining it', and later continues: 'It would not be correct, however, to represent chemistry as a science where theory is negligible.' There could be little doubt that the first statement must surely apply to physicists at least equally with chemists, but a serious difficulty, and one which is perhaps not always fully appreciated, seems to arise in their differing conceptions of what a *theory* is. I have come across this situation in practice where such topics as semi-conductors are involved. This field attracts quite fundamental physicists, on the one hand, through such disciplines as statistical mechanics, thermodynamics and electron transport phenomena, while on the other hand the problems of bonding between atoms through the crystal and the importance of various crystal structures, a field often referred to as crystal chemistry, naturally involve chemists. The exasperation which not infrequently arises in discussions, or when a physicist may be trying to assess a paper written by a chemist in this field, has seemed to me in the past to boil down essentially to this rather different conception by each of the essence of a theory and its function.

It does appear that on the whole a physicist (or perhaps better a so-called pure physicist, because probably applied physics lies nearer to what Caldin feels a chemist does) tries to *reduce* his theory at all times to as few parameters as possible and is inclined to feel that a theory is a 'respectable' one, though by no means necessarily correct, if in principle it does offer reasonably specific means for its possible refutation. Moreover the physicist will generally arouse irritation amongst fellow physicists if he is not prepared to abandon his theory when it clashes with subsequent experiments. On the other hand it would appear that the chemist regards theories—or perhaps better *his* theories (!)—as far less sacrosanct, and perhaps in extreme cases is prepared to modify them *continually* as each bit of new experimental evidence comes in. The chemist is surely quite right in believing that such theories or 'hypotheses' will often be very valuable in guiding further research programmes; one recalls the phrase: 'A bad theory is better than no theory at all', or as Caldin says: 'If the term "hypothesis" is used broadly enough, so that it covers the guesses that inspire research

programmes. . . . . ' On the other hand, when the physicist gets involved in this, I think he frequently gets smitten with a vague, but rather deep, sense of unease, because he finds it difficult to see how a theory in this sense can ever be proved wrong if the chemist, in the sense I have just used, is always prepared to modify his theory to bring it in line with the latest ' stop press ' experiments.

Does this simply suggest that one must beware of trying to lump all scientific activities together, or more particularly, that the analyst or philosopher of science must beware of so doing? On the other hand, it suggests to me personally that the practising physicist or chemist himself might occasionally profit by recognising the rather different methods that each may adopt—largely, one presumes, because the chemist is generally prepared to look at and think about much more complicated situations involving many more physical parameters than the physicist is generally prepared to do. At the same time one calls to mind the remark that: ' When the biologist has reduced his problem to chemistry, he says that it is solved; when the chemist reduces his problem to physics, he says it is solved; but how on earth does the physicist know when his problem is solved ?'

D. K. C. MacDonald

National Research Council,
Ottawa, Canada

REVIEWS

THE PROBLEMS OF PERCEPTION

1 *Introduction*

THE traditional ' philosophical ' problems of perception have, in the past, been dealt with by two different groups of people, professional philosophers and neurologists,[1] between whom there has been little liaison. In the last few years, however, certain neurologists such as Brain,[2] Kuhlenbeck,[3] and Smythies,[4] have attempted to reconcile these two fields by developing the traditional neurological approach to perception to a point where such a correlation has become possible and by making surveys of philosophical theories from this position. Recently a philosopher—R. J. Hirst [5] has performed the complementary task of examining the arguments of both other philosophers and neurologists about perception. His aim is to construct a comprehensive theory of perception that will cover all the facts systematically, which will ' answer the questions traditionally and justifiably raised and which will reconcile customary belief, psychological analysis and scientific findings ' (p. 16). This work is of great importance for it represents the first attempt by a philosopher to come to grips with *modern* neurological ideas about perception and the mind-brain relation. Most modern philosophers who have written on this subject have paid scant attention to the views of the neurologists. They held that neither the facts nor theories of the neurologists had much bearing on these traditional problems which they felt arose out of a misuse of language or from the purely technical errors of previous philosophers. However Hirst makes it plain that this will no longer do:

> . . . the scientific account of perceiving cannot simply be dismissed as complementary and innocuous to ordinary notions. It claims superiority, and if it is correct it undermines common-sense beliefs and the ' ordinary language ' which presupposes them (p. 319).

[1] Used in an extended sense to include varieties of neurobiologists as well as clinical neurologists.

[2] Sir Russell Brain, *Mind, Perception and Science*, Oxford, 1951, and *The Nature of Experience*, London, 1959

[3] Hartwig Kuhlenbeck, *Brain and Consciousness*, Basel, 1957 and ' The meaning of postulational psycho-physical parallelism ' *Brain*, 1958, **81**, 588

[4] J. R. Smythies, *Analysis of Perception*, London, 1956

[5] R. J. Hirst, *The Problems of Perception*, London, 1959

> Nor is it [the attempt to construct a comprehensive theory of perception] a shadow battle with pseudo-problems: for as ordinary language is shaped by or reflects common-sense notions about the nature of things, to show that certain perplexities ' dissolve ' when due attention is paid to its logic is simply to show that they do not arise for common sense, untutored and unscientific as that may be. Thus the essential point is missed that the problems of perception are problems largely because a study of the psychology and physiology of perception and of the characteristics of illusions and hallucinations seems to call for a radical revision of the plain man's assumptions (p. 16).

Hirst's main task is to lay out certain assumptions about perception made by commonsense and then to see how and to what extent these assumptions are threatened by neurological facts and theories or by philosophers, such as Broad and Price, in the same general camp as the philosophising neurologists.

2 *The Sense-Datum Theory*

In general we can say that Hirst is much more sympathetic to the representative theory than is the majority of philosophers and his own theory has much in common with this theory. Naturally enough he starts off with a consideration of the sense-datum theory. Here the only point that I wish to make is that most of the difficulties that he finds in the traditional arguments for sense-data (see pp. 26–37) arise from the definitions of ' sense-datum ' given by Price and others in terms of ' knowledge ' or ' certainty ', or on ideas about ' direct apprehension ' or ' what is given to consciousness ', or on premiss-conclusion types of argument such as the argument from differential certainty.[1] However, alternative definitions have been based on such ostensive properties of the visual field as the relation between sense-data and after-images which have been claimed by Moore [2] and Smythies [3] to be observable on introspection. These definitions seem to avoid these difficulties and they enable us to use the sense-datum theory without being forced to engage in perpetual skirmishes about the epistemological soundness of our foundations. Hirst then goes on to examine certain difficulties in the concept of sense-data, the first being the dilemma of attention—the apparent need to assume an unvaryingly excellent awareness in sensing. It is argued that if we define sense-data along lines such as were used by Price in terms of what is certain in perception, then our awareness of sense-data must always be clear cut enough to preclude doubt. This does not seem, however, to tally with the introspective facts and leads to certain awkward logical consequences. Again this difficulty arises from the vulnerable epistemological definition of sense-data and if we accept instead the

[1] See Hirst, op. cit., p. 32

[2] G. E. Moore in *British Philosophy in the Mid-Century* (C. A. Mace, Ed.), London, 1957

[3] Smythies, op. cit.

phenomenological definition I advocate [1] we can then say, without refuting our own premiss, that sense-data can be either clear or fuzzy (as when I take my glasses off) and awareness can be clear or fuzzy too (as when I am drowsy or giddy). I have dealt elsewhere [2] with the further problems that he raises in this section about sense-data—i.e. their location, category, and relation to material things and these points will be commented on later when I discuss my own theory in the light of Hirst's criticisms. He then deals, in the next two chapters, with phenomenalism and with linguistic methods in philosophy and my only comment here, for what it is worth, is to agree with nearly everything that he says.

3 *The Representative Theory*

So we come to the traditional neurological account of perception—the representative theory. Hirst first gives an account of modern statements of the theory as made by Eccles, Brain, and myself, and he then brings arguments against it. However his statement of the theory itself contains a number of slips and his arguments will not bear the weight of refutation that he has placed upon them. I will deal first with the errors that he makes in presenting the theory. He claims that the representative theory is objectionable because it leads to a duplication of perception. If we do not see objects but see only sense-data (percepts) the problem seems to arise how to account for this second internal perception. Why, he asks, if external perception needs brains and sense-organs, does not this internal perception need them too? This seems to lead to absurdities about 'little men in the head' (see, e.g. p. 280) or to a vicious infinite regress. These objections may be answered as follows. His error is to suppose that the representative theory states that we 'see percepts' or have a 'perception of percepts' or that it postulates a 'new perception of sensations'. Modern versions of the theory make it plain that we need not say that we *see* or *perceive* percepts, sensations, or sense-data. We should say that we *have* sensations or percepts or that we *sense* [3] sense-data, and that we *see* or *perceive physical objects* by means of the part-whole relation holding between sensing and perception. That is, the theory states that *sensing* is the final event in any perception—that which takes place in consciousness—and perception includes sensing and, in addition, a whole complex of physical and physiological events *outside* consciousness.

[1] Smythies, op. cit. [2] Smythies, op. cit.

[3] We can say 'what you do when you are inspecting (examining, watching, observing) an after-image or eidetic image or mescaline hallucination is sensing and whatever is common between these activities and the perception of a physical object is sensing' (this is of course Moore's definition).

The further development of this point will also answer another of his objections. This is the familiar epistemological criticism of the representative theory ' if you can only observe sense-data how do you know that there are any external physical objects to cause them ? ' This question can only be answered, as Hirst correctly asserts, by the ' best explanation of the world ' hypothesis and by abandoning the impossible ambition of finding a logical proof of the existence of the external world. The further question then arises that, granting the existence of such a world, how could private sense-data mediate any information about the characteristics of such a world ? This may be answered by a proper attention to the logic of representation. It is often assumed that the information content of any set of objects or events always refers primarily to the characteristics of these objects or events, i.e. the fact that an object is round or brown is held to be primarily a fact about that object itself.

In order to develop this point we must first distinguish between *symbolical* and *non-symbolical* representative mechanisms. A representative mechanism is one that transmits information. It may do this either by means of a signal code (symbols) as in the case of the telegraph or semaphore, or without such a code, as in the case of television and the cinema. It is a characteristic feature of the latter type that observation of one set of events gives us direct non-inferential information about another set of events not observed directly (common-sensical ' directly ') at the time. The representative theory of perception holds that, in the sense that the pictures on the television screen are non-symbolical representations of what is going on in the studio, sense-data are non-symbolical representations in consciousness of what is going on in the external world. Hence it would not be correct to talk about sense-data as *symbols* of their external causes as Hirst does on page 279. Philosophers always seem uneasy when television is advanced as a model of certain aspects of perception. However the word *is* tele*vision* and when we watch television we gain information about the events televised, although we only observe (strictly) a pattern made by lines of light on a screen. No process of inference, deduction, or decoding is involved and nor does what we *see* depend upon any suppressed inferences derived from our knowledge of how television works. A savage watching television would *see* lions and warriors and spears (although magical *explanations* of this unusual seeing would doubtless be advanced on a closer inspection of the situation, the mode of his explanation would not alter the fact that he was seeing the lions, etc.) Very small children accept television completely without knowing how it works. Therefore television provides a sound model of the relation of sensing to perception from an epistemological viewpoint, and shows us how sense-data can mediate facts about the external world.

The adoption of this model leads, however, to the difficulties concerning the further explanation of the processes involved that Hirst brings forward.

He says that perception needs eyes and brains and as we need eyes and brains to see the picture on the T.V. screen, therefore we ought to have ' internal ' eyes and brains to see the sense-data themselves—which seems ridiculous both in itself and because it leads to an infinite regress.

The psychological reasons for bringing forward this argument seem to me to be as follows. We have for so long considered that sensing is the whole of perception (as a simple direct acquaintance with objects) and to contain within itself the whole causal chain of perception that, if we consider that the representative theory states that sensing is only the final outcome of this causal chain, we feel that, if this is so, then there must be another ghostly causal chain to replace the physiological chain that has been excluded [1] from the reach of introspection. This feeling is, however, merely an irrational hangover from our old habits of thought, for there is no reason why *perception* should not require an elaborate physiological apparatus (to conduct these processes of perception that take place outside the field of consciousness to consciousness) yet *sensing*—the immediate prehension of sense-data available to introspection—should require none. It may merely be the property or activity of the Pure Ego—' that which in us perceives and thinks '—not needing any such apparatus.

The complete account would run as follows. There is perception of physical objects and a part of this is composed of the physiological causal chain of events that culminates in the construction of a sensory field. The other part is the final (non-physiological) event of this chain—the sensing of this sense-field by a Pure Ego.

There is no reason why this final event should in turn be divisible into another causal chain, nor why explanations found to hold in the case of perception *must* also fit sensing, nor can we give any further explanation of sensing except to say that it occurs and that our evidence that it does so is introspective. However, any causal explanation soon reduces to terms that are ultimate for which no explanations can in turn be found, so this is no embarrassment. To explain an event is to find within it further links in the causal chain or it is to demonstrate hitherto unknown external relations to other events. Sensing cannot, on the introspective evidence, be subdivided and it relates only a Pure Ego to sense-data and sensory images. The brain events correlated with sensing can of course be (and are being) elucidated but these are external relations of the process. To suppose that sensing

[1] This exclusion arose because representative theorists claimed that the events of the physiological causal chain occur both temporally prior to the construction of the final percept and that they take place out of the reach of introspection: see in this regard the accounts given by Fessard and Lashley in the recent symposium on Brain Mechanisms and Consciousness (J. F. Delafresnaye, ed., Oxford, 1954) and my discussion of this symposium in *Brain* (' Brain Mechanisms and Logic ', 1957, **80**, 393).

must be divisible into a causal chain is to apply irrelevant criteria of explanation.

The logical point can be generalised. We may consider that we are dealing with a set of events A with a set of characteristics X. It is then postulated that the set A really consists of two subsets a and a' and that most people have previously misidentified A with a. It is further suggested that some of these characteristics X belong to a (as x) and some to a' (as x'). We then have no cause to insist that x' must characterise c, and we must realise that our compulsion to do so arises from the strength of our previous undoubting but yet erroneous identification of a with A. To apply this argument in this case substitute for A ' the events involved in perceiving an object ', for a ' those parts of A available to introspection ', and for a' ' those parts of A not so available '. X, x and x' are merely the properties of A, a and a' respectively and in this case only x' needs to be specified, i.e. ' forming a physiological causal chain '.

Hence the modern representative theory neither duplicates perception nor is there any reason to accept Hirst's claim that we must press the T.V. analogy to account for sensing too. Nor does the representative theory accuse ' us all of ineradicable and inexplicable error ' (p. 179). We certainly do see objects and perceive the events of the external world and the representative theory does not have to say that we do not. Normal perception includes sensing as part but of course sensing can also take place without perception (as in hallucination). Furthermore normal everyday perception and sensing are associated with a particular psychological set and content of thought (in terms of evaluation of and action directed towards the external world). However, even when we are looking at everyday physical objects we can adopt what Quinton [1] terms the phenomenological frame of mind (if we are philosophers trying to discover sense-data, or psychologists interested in constancy). All that is different here is that we adopt a different psychological set and content of thought (directed towards a phenomenological analysis of the visual field). In other words our thoughts and actions are different in each case.

One last point in this section: Hirst asks (p. 174) ' . . . what is the evidence for this novel mental seeing ?' Common usage gives us one. So expressive a phrase as ' the mind's eye ' could hardly have the widespread use it does if there was not something very like seeing about having sensory mental images (I use this term to include eidetic, hypnagogic, and mescaline images).

4 *Mr Hirst's Aspect Theory of Perception*

So all the reasons given for rejecting the representative theory arise either out of Hirst's mistakes in presenting the theory, out of the fact that

[1] A. M. Quinton, ' The Problem of Perception ', *Mind*, 1955, **64**, 28

his conclusions do not follow from his premisses, because he confuses the logic of symbolism with the logic of representation and because he presses the ' television ' analogy of perception too far. This considerably weakens his motives for constructing a rival theory but I will now examine this on its own merits. The central notion of his theory is that of ' aspect '. Its use is introduced by six analogies each of which illustrates that those characteristics of an object or situation that we can observe at any one time depend on our point of view and even more so on our mode of access to the object. The simpler analogy is seeing two sides of a building. Modes of access are illustrated by hearing and seeing an accident or more closely by witnessing an accident and actually being in one yourself. A further example is the difference between thinking of a ray of red light and of a train of waves at 700 mμ. The point of using ' aspect ' is ' to indicate the limitations of each mode of access, namely that on it alone one cannot be aware of or ascertain all the characteristics of the thing ' (p. 189). But, he goes on to say, we must take care not to reify aspects (this is alleged to be the main error of the sense-datum theory) and he emphasises that to be aware of an aspect of a thing is to be aware of that thing itself. This notion is then applied to perception as follows. In perception there are three modes of access to the events taking place. The *inner aspect* is the aspect of the percipient himself—the content of consciousness when we perceive a physical object (p. 220, line 12). The *outer aspect* is the physical and physiological causal chain terminating in a certain brain state. The *third aspect* is taken up by the philosopher who seeks to correlate the inner and outer aspects. From this latter aspect the other two are ' identical in the general sense that they are the one relation or situation of perceiving viewed in different ways or as revealed on two different modes of access ': (p. 286).

Hirst insists that we do perceive physical objects and he wholly rejects the sense-datum account of private percepts. The relationship between the brain events in perception and consciousness is dealt with as follows. One finds that ' a given content of perception ' is closely linked with certain brain activity which is its necessary condition. But this should not lead us to suppose that there is a ' strict identity ' between the outer and inner aspect of perception. The content of perception is the whole inner aspect, but the brain activity is only a part of the outer aspect (the rest being causal chains emanating from a physical object). This consideration leads Hirst to postulate ' perceptual consciousness ', which is itself a whole with two modes of access. Its inner aspect is the ' same in character ' as the inner aspect of perceiving but its outer aspect is brain activity and is thus only a part of the outer aspect of perceiving. The inner and outer aspects of perceptual consciousness can, according to Hirst, be strictly identified in a particularising sense, unlike the inner and outer aspects of perception that can only be

identified in the very general sense in which aspects are identifiable and not in any particularising sense.

This scheme may be commented upon as follows. The distinction between identity in only a very general sense based on aspects and identity in a strict particularising sense is going to lead to trouble. The whole point of introducing aspects was that the events of which they are aspects are identical. There is strictly only one object or set of events or situation which present two aspects. Hirst defines the outer aspect of the perception of an object as identical with the whole causal chain (p. 288). But he identifies its inner aspect only with a part of this causal chain, the brain activity. The clause he brings in to save the situation is that the inner and outer aspects of perception are identical only in the sense in which aspects are identifiable. However, the difficulty here is not merely that two aspects of one set of events are different but that the two alleged aspects contain different events—brain events only in one case and brain plus external physical events in the other. Hirst holds that the inner aspect of perception is identical with the inner aspect of perceptual consciousness (in veridical perception) (p. 220 ' Perceptual consciousness is normally part of perceiving . . . '). The outer aspect of perceptual consciousness is certain brain events. The outer aspect of perception comprises the same brain events plus other physical events external to the body. Yet Hirst supposes that two aspects of one set of events or of one situation are merely two views of one and the same set of events. Clearly something has to give way here. Two aspects of one object, set of events, or situation can of course be said to contain different events as in the case of two views of one building from the north and south or in the sense that California and Maine are aspects of America.

There are thus two uses of ' aspect ' that Hirst does not distinguish very clearly, i.e. two aspects of (rigidly) one set of events or object (such as two views of one surface of a penny) and two aspects that are two different sets of events united only by being parts of some whole (such as the examples given above). The relationship between the inner and outer aspects of perceptual consciousness uses ' aspect ' in the first sense and that between the inner and outer aspect of perception uses ' aspect ' in the second sense. However, only the use of ' aspect ' in the first sense will support the claim that physical objects are directly given in experience (which requires the strict sense of identity between perceptual object and physical object and thus that the inner and outer aspects of perception should be strictly identical): the use of ' aspect ' in the second sense will only support the claim that there is some classification in which the content of a perception and the physical object may be regarded as parts of a common whole. But this claim is so weak as to be satisfied by almost any theory of perception and Hirst's whole elaborate theory is stultified. Hirst's use of the notion of modes of access is discussed further below.

5 *Further Criticism of Hirst's Thesis*

There are some further points to discuss about his account of perception.

(4.1) The three modes of access seem somewhat artificial. Admittedly I alone can have my experiences and my nerve impulses are public to anyone with a drill, electrodes, and an oscilloscope. Yet I can observe the nerve impulses in my own brain in a public fashion (under a local anaesthetic such as is commonly used in neurosurgery) and I can certainly do my own philosophising. Therefore the epistemological basis of the three modes of access that is so important for his theory seems much less clearcut than Hirst makes out. There seems to be a further confusion here over ' points of view '. At first Hirst uses this to explain why the contents of my consciousness should appear so different from the state of my brain with which they are supposed to be identical. The pill is made easier to swallow by the examples that show how things can look different from different points of view. ' Point of view ' here implies actual perception. However Hirst soon slides into another sense of ' point of view ' where it means ' opinion ' or ' as considered by a person with special interests and training '. Clearly the third or correlator's ' point of view ' is of the latter kind (although the switch is made without any warning of the very different logic of the two meanings). Hirst uses ' aspect ' both to cover the original phenomenological introduction of perceptual consciousness (as a normal component of every perception) and as ' a theoretical notion, something conceived from that third point of view . . . '. Yet it hardly seems plausible to maintain that, when I am perceptually conscious of some sensory image, all this is the theoretical notion of some hypothetical correlator. However Hirst gets round this by ' points of view ' again. My perceptual consciousness is act/object in character *to me* (on its inner aspect) but from the correlator's point of view it is ' adverbial '.

This distinction is based, however, upon an error of exposition. Hirst states that ' . . . the whole point of distinguishing perceptual consciousness from perception is that it is supposed to be capable of occurring without causation by an external object ' (p. 289). His whole argument depends on this point—that epistemological considerations about hallucinations are the *only* reasons for distinguishing between perceptual consciousness and perception. But on page 287 he gives *two* reasons for distinguishing between perceptual consciousness and perception—this one and another concerning the disparity in space and time between our percepts and the objects of which they are percepts, arising out of the finite velocity of light. In this latter case he is forced to introduce *objects* of perception (thinly disguised as ' a given content of perception ' (p. 286)) that are on *our* side of the spatio-temporal gap between us and physical objects.

In any case the whole notion of an adverbial experience seems fantastic. This is the notion that I do not experience, e.g. red eidetic images but that

I am merely experiencing redly ! It also seems odd that the same event can both be adverbial and act/object in character (which by any ordinary criteria are mutually exclusive) no matter how we juggle our modes of access. If one has a red round after-image it seems strange to claim that one is merely experiencing (no *thing*) redly, roundly, and presumably ' somewhat-at-a-distancely ' instead of saying simply that one has (can introspect, observe, etc.) a red round after-image set somewhat at a distance. We cannot fall back on the concept of ' projection ' of images as Hirst shows symptoms of doing (p. 38) since this has been shown to be a myth.[1]

His distinction between adverbial and act/object modes of experience is based not only upon this error of exposition but also upon a circular argument—to wit : Hirst seeks to prove that perceptual consciousness is act/object in character from the percipient's viewpoint but is adverbial from the correlator's by saying this is so because

(i) no distinction can be made between act and object from outside: for

(ii) the whole point (*sic*) of distinguishing perceptual consciousness from perception is to allow for the experience of hallucinations :

(iii) in hallucinations there cannot be any true act/object relation because no true object is present:

(iv) this is so because the postulation of the existence of any ' true ' objects such as sense-data presupposes the evidence and point of view available only to the correlator because

(v) as soon as one considers physiological processes or attempts to determine the existence of anything one leaves the inner aspect on which alone perceptual consciousness is act/object in character and one must therefore reassess it and take up the correlator's viewpoint—which is

(vi) that perceptual consciousness is adverbial in character—thus completing the circular ' proof '.

(4.2) The ordinary notion of ' aspect ' or ' point of view ' presupposes some observer (even if only a mechanism such as a camera and even if only a hypothetical one) at the nodal point of the aspect. In the case of Hirst's ' outer ' aspect the observer is clearly the person of the physiologist. But what is supposed to fill this role in the case of the ' inner ' aspect? Hirst cannot on his own admissions accept any such entity as a Self or Pure Ego. Yet to what else could the word ' their ' refer (on p. 216, line 24), where the ostensive reference is to human beings who are attributed with ' privileged access ' to their own thinkings (*sic*). The concept of ' privileged access logically presupposes the existence of some entity to exercise this access to

[1] J. R. Smythies, ' Analysis of Projection ', this *Journal*, 1954, 5, 120

thoughts, i.e. an Ego. Does, then, one part of the brain view the other parts? Or do the physiological mechanisms of final sensory analysis and attention merely constitute the outer aspect of what appears on the inner aspect as an awareness of sensations and images? But this does not dispose of the point that the logic of aspect, even of ' inner aspect ', requires some x for the aspect to be an aspect *to*. The nature of this x remains very obscure and one is left with the uneasy feeling that Hirst is trying to have his cake and eat it by strenuously denying the existence of a Pure Ego and yet putting the fellow to work in his theory under a number of disguises.

The reason why Hirst introduces these clumsy and dubious ' aspects ' is to try to maintain a self-contradictory position—i.e. that the content of our consciousness and the events in our brains have quite different characteristics and are yet identical.

However, even if we allow his doctrine of aspects and agree to overlook the awkward point about to whom or to what the ' inner aspect ' of our perceptions are directed (views are views *to* or *from*, as well as views *of*), this reprieve is soon revoked by another major technical flaw in his account. Even if we grant that the inner and outer modes of access to perception will mean a great difference in the characteristics of the aspects revealed one cannot assume that this difference is limitless in range. Certain differences between what are apparently two sets of events may be due to the fact that they are really only two different aspects of one set of events but other differences clearly cannot be so. There are limitations in this that one must explore more closely than Hirst has done. One of these limits is set by topology. Hirst's reply to the point made by Brain and myself—that there is nothing circular about the events in my brain when I am perceptually conscious of a circle—is that identity ' only presupposes congruence when events are arranged in the *same* overall spatial order or frame of reference ' (p. 301). He then goes on to claim that what is ' physically one and the same object or event may have a position and dimensions in two different spatial orders, and there will then be no congruence between the shapes and dimensions in the one and those in the other, though there may well be a correspondence ' (p. 301). An object may of course have different positions and dimensions in two different spatial orders or frames of reference but it will not have two *completely* different shapes. If we choose sufficiently peculiar frames of reference we can turn a circle into an ellipse or into a number of other closed curves. But it seems to me that mere juggling with our frames of reference cannot change a circle into a member of a topologically distinct species of curve such as an open curve, ring or even two circles without laying us open to the objection that we were no longer presenting an aspect of *one circle* even granted the utmost latitude in our interpretation of the meaning of ' aspect '. In the case of our percepts and the brain states correlated with them, the transformation (as Brain graphically

describes) is from *two* neural schemata shaped as inverted and distorted semi-circles (open curves) in widely separated parts of the brain to the one single circle in consciousness. Yet these two are alleged to be identical. The representative theory, as I have constructed it, does not have to wrestle with such difficult problems, for the percept and the brain state are held to be *different* sets of events and so they can be quite different shapes since the only relation between them is causal (or, in my Theory II, *external* spatial as well).

There is a further difficulty in Hirst's theory. To illustrate his point Hirst uses the example of a penny that looks round from one place and elliptical from another. Thus two aspects of one object need not be congruent. However, although the penny *looks* round from one place and *looks* elliptical from another, it is also true to say that it *is* round (determined by repeated scientific measurements in the manner that Hirst describes (p. 166)). Now the brain events concerned in a perception (or in perceptual consciousness) are x-shaped and the content of the perception is y-shaped. What then is the shape of the one set of events of which they are both aspects? It cannot be both x-shaped and y-shaped. Therefore it must itself be either x-shaped or y-shaped or even quite another shape (the penny can really be round yet it can present two aspects that are different ellipses, or even an ellipse and a rod-shape, as when it is seen on end), or perhaps it does not have any shape at all. Hirst's criterion for settling the real shape of an object is by scientific measurement (p. 166-169). Applied in this instance it would give us the answer x-shaped. This would, however, give the content of our consciousness the epistemological status of a mere visual illusion.[1] This brings to light another complexity in Hirst's theory and that is the problem of the real nature of the events of which both the brain states as described by the physiologist and the experienced events introspected by the percipient are supposed to be but aspects. The only clue here is that Hirst says that it is perceiving itself that presents these two aspects. Perceiving is defined (p. 285) as a *relation* between a person and other public objects in which he is aware of them and some of their characteristics. But perceiving as a relation could hardly be said to have any *shape*. Yet its aspects have shapes. But perhaps we are pressing the penny analogy too far. The penny presents two aspects that have shapes and the penny has a shape itself, but perhaps perception presents aspects that have shapes yet it has no shape itself. Clearly the confusion arises out of the use of the two different meanings of 'aspect' described above—the phenomenological use and its use to set limits of discourse. However, since the penny analogy depends on the use of 'aspect' in the first sense and Hirst's account of perception rests on the use of 'aspect' in its second sense, it is not at all clear that this analogy can be

[1] i.e. a distortion and not an illusion (= illusory) in Hirst's sense (p. 215, line 15) for the *apparent* shape in consciousness is not the *real* shape of the events concerned.

used to illustrate positions that have arisen in this latter analysis based on the use of quite a different sense of ' aspect '. That is to say, the difference between a brain state and a content of consciousness might *appear* to be like that between that of the two views of the penny but the logic of ' aspect ' is quite different in each case and this difference is brought to light by asking of what are our brain states and the content of our perceptions supposed to be aspects. The same problem arises in the case of perceptual consciousness which is defined as ' a whole activity of the person presenting aspects on the two main modes of access ' (p. 287). Its aspects have shapes but it apparently has none being a ' whole activity ' of the person.

What is going on here seems to be something like this. Let us consider the Stock Exchange. This presents various aspects. It presents two types of internal aspect—what its rooms and corridors look like from inside, and what it is like to be a member of the Stock Exchange, or ' aspect ' might refer to details of its internal organisation. Outer aspects of the Stock Exchange can also mean either what it looks like from the various streets around it or what impact or impression the functional organisation of the Stock Exchange has upon a body like the Treasury or the Bank of England. Clearly, if we use the one word ' aspect ', or its close relative ' point of view ', to cover all these different components with their widely varying logic, the results of our researches will not be as clear as they might have been. In the terms of our analogy of the Stock Exchange, the representative theorist would like to say that the problem arises because the arrangement of the rooms and furniture is not the same as seen from these rooms themselves as it is when we look in through the windows from the street outside. Hirst's method is to say that the Exchange is a whole activity of an organism that presents aspects. But the aspects that he describes, i.e. its aspects to the Treasury and to its own members are not those proper to this context—which are, on account of the spatio-temporal relations involved in perception, its appearance from the street and from inside its own rooms.

6 *Conclusion*

Thus in conclusion one can state that Hirst's arguments against the representative theory, although most thorough and detailed and exhibiting a sophisticated acquaintance with modern neurology and psychology, are invalid. They may be answered as follows:

(i) ' . . . the notorious and unsolved problem of how brain activity can cause something so radically different as an experience.' This should be put ' The brain activity causes changes in the content of our experience (in our sensory and image fields) '. There is no *a priori* reason why there should not be causal relations between events in physical space and events in

sensible space as postulated by Price [1] and by myself. The idea may be novel but where is the paradox? At least Hirst does not produce one in discussing this particular point (p. 179, line 1) where he claims the idea is a ' paradoxical novelty '. He only says that all scientifically recognised causal relations hold between entities in the same spatio-temporal system. However no *radically* new scientific concept is recognised at the time of its conception. It usually takes a very long time even if it is any good. The previous difficulties lay in trying to imagine causal relations between extended and unextended entities. Another lay in trying to think of the brain churning out images and sensations from its own mechanism. My theory involves neither of these two difficulties.

(ii) The claim that the representative theory duplicates perception is based on a mis-statement of the theory, at least in its most recent form, and the further claim that it leads to ideas about little men in our heads or to an infinite regress arises out of pressing an analogy too far.

(iii) The claim that it is weak to postulate a host of private entities may be met by pointing out that the question of whether there are any private mental entities or not is a question of fact (as Hirst himself recognises on p. 117). The first business of science is natural history—to make catalogues of whatever particular existents there may be and at this point questions of logical elegance do not arise, however important they may be at later stages of the scientific game. Furthermore on his own theory he needs three modes of access to perception, one of which is subdivided into two modes of awareness (p. 190, lines 21-22) and only two of which have any evident correlation with the facts of the matter. He also needs two modes of identity, the slippery distinction between perception and perceptual consciousness, two meanings of ' access ' and, moreover, sense-data keep creeping in clad in various disguises such as ' a given content of perception ' (p. 286) or ' the content of perceptual consciousness ' (p. 295). This complicated theory needs to be set against the simplicity of the representative theory which describes only sensing and perception standing in part-whole relation and their respective objects—sense-data and physical objects. Hirst's first two ' modes of access ' can be derived from a consideration of the spatial relations between sense-data and physical objects and they are not used as basic coinage of explanation. His third mode is abolished altogether. The concept of ' access ' is not used and only one manner of being identical is recognised.

I do not think it any exaggeration to say that this is the most important book on perception written by a philosopher since Price's *Perception*. Its critical portions should do much to close some overdue philosophical accounts

[1] H. H. Price, ' Survival and the Idea of " Another World " ', *Proc. Soc. Psychical Research*, 1953, **50**, 1

and its constructive arguments should set the form of philosophical debate in this field for the next decade.

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J. R. SMYTHIES

The Worcester Foundation,
Shrewsbury, Mass.

WITTGENSTEIN'S THEORY AND PRACTICE OF PHILOSOPHY

A MOST striking fact about these two books[1] is this: many people (including myself) are keenly disappointed by reading them, while dipping into them gives great pleasure. I think this split remains even if one both discounts the aesthetic pleasure of lively and forceful prose and remembers that these books are preliminary studies, intended for limited circulation. Another thing is that they make a vastly superior impression, at least on me, compared with Wittgenstein's remarks on mathematics.[2] This too remains after one takes into account that the latter were never meant (by him) even for limited circulation.

In other words, many people do not get from the books what they are led to believe from dipping into them. Wittgenstein himself repeatedly and quite explicitly states what he has to offer (or, at least, what his aims are) in often quoted sayings about letting flies out of fly bottles, etc. Below I shall not try to relate the content of the books to these aims, because I think they are bad ones. Instead, after trying to convey something of the first impression, I shall try to see what emerges from these books for philosophy as a discipline in its own right, or, better, as several such disciplines. I do not believe that the foundations for such a discipline are yet laid, and hence much of what I have to say may well turn out to be short-sighted.

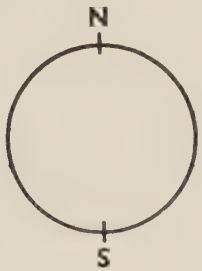
Clarification of Concepts. The book contains descriptions of the use of (ordinary) language in sharply circumscribed circumstances, the so-called language games, and other imagined situations (*Gedankenexperimente*). Many of them illustrate vividly (i) possibilities that may easily be overlooked when one hears for the first time traditional philosophical problems and views or (ii) associations which they perhaps unconsciously, arouse. Before one is surfeited by the torrents of examples, the impression is surely this:

[1] Ludwig Wittgenstein, *The Blue and Brown Books*, Basil Blackwell, Oxford, 1958. Pp. xiv + 185. 25s.

[2] *Remarks on the Foundations of Mathematics.* The review in this *Journal*, 1958, 9 will be referred to as *RFM*.

Here we have a *whole* subject comparable to a development of physics that consisted of nothing but its epoch-making highlights like Einstein's considerations on the measurement of time or Heisenberg's on the simultaneous measurement of position and velocity. No dreary experiments with safeguards against artifacts, no out-of-the-way information for which one needs an elaborate technique, no formal apparatus with paralysing definitions.

It seems to me that much of this would also interest an educated person without special interest in the traditional problems of philosophy such as the nature of our logical concepts or in the reductionist theses of positivism and nominalism. In particular, he will be impressed, or even overimpressed, by the shock effect of unexpected situations. This effect, from which many of Wittgenstein's examples derive a good deal of their force, is, of course, familiar. It seems instructive to consider it by taking a hackneyed example.



Suppose someone (in England) objects that the earth can't be round because then the Australians would fall into space. Wittgenstein would turn the page upside down and say: now *we* fall into space.

The argument might, I suppose, be called philosophical (what else?) where philosophy acts as a handmaiden who does the clearing up in other fields of study, in contrast to being a subject in its own right. This is surely an example of what he had in mind when saying that philosophy clears up misconceptions as one clears away rubbish before putting up a building: in this case something is indeed to be put up, namely the concept of a round earth.

But, more important, the example illustrates how he would leave a problem at an initial stage without enquiring on what assumption the force of the argument rests, namely symmetry of the situation about the centre. Such a second stage might well be important in further understanding of gravitation. Lastly, of course, the argument would be a game if the earth were not round or the Australians fell off.

But even though the example is only a small step in a fuller study, here we have such a study too, while in most cases in these books this is not the case. So one is not in a position to see whether the shock is salutary or

paralysing. As an example of the latter we shall consider below (just before the *Summary* at the end) a case in the foundations of mathematics. Here I shall use a simpler imaginary[1] example for illustration.

Suppose in some elementary experiments on water and rust one had found that both of them gave off oxygen, and so concluded that they must contain oxygen. Would Wittgenstein not have made fun of it saying that there were no bubbles in the water, etc.? Nobody had thought of bubbles, and hearing it at the very beginning of research in the subject, one might well be shocked into believing that one had overlooked something.

This shows two things. First, when the subject is developed and more is known, the puzzle takes care of itself. One might even agree that ' contain ' is not quite the right word. But, second, at the early stage the shock would be merely confusing; since many of the philosophical problems are at this stage, one must not be too impressed by unexpected situations.

In view of this, one no longer accepts the shock as proof that ' something is radically wrong ', but wants to know what ' building ' is to be put up. For instance, there is (or, at least, was) a certain shock in the examples which show that, often, when we say ' I mean X ' no particular mental act of meaning is involved, or that sometimes a substantive does not denote a material object or a sensation. We have overlooked something; but was it serious to overlook it as in our first example or trivial (i.e. right not to bother about it) as in our second? Wittgenstein does not tell us for which further study this clarification is to make room.

I shall consider the obvious reductionist theses suggested by these examples.

The reader is warned that some illustrations are taken from the philosophy of mathematics. This is regrettable[2] because it is probably not the potentially most fruitful field of philosophy at the present stage. It is unavoidable, not only because I happen to know a little better what is done in this field, but also because objectively the most substantial work is done in it at present.

Traditional points of View. I shall try to make out that quite natural developments of Wittgenstein's considerations may be formulated as a *reduction to the concrete;* for want of a better term I shall call it semi-behaviourism (with respect to mental acts) or semi-nominalism (with respect to abstract objects).[3] This attempt seems quite perverse because Wittgenstein is explicitly and categorically opposed to any reductionism, and contrasts the latter with a philosophy that is ' purely descriptive ' (p. 18), describing,

[1] The example seems to me apt, although there is at least one passage to contradict this impression. On p. 35 Wittgenstein is quite uncritical of the question, ' What are the ultimate constituents of matter? ' However, the case below does not depend on this since we do have the example from the philosophy of mathematics just mentioned.

[2] Except in the section on Wittgenstein's theoretical position, where it is apt.

[3] An elegant summary of the outlook is given in Bernays' interesting and enjoyable review of the *Remarks on the Foundations of Mathematics*, in *Ratio* 1959. A somewhat related summary is buried on p. 136 (1.2*d*), p. 142 (2.3*a*), p. 144 (3.3) of *RFM*.

as he says repeatedly, the grammatical[1] rôle of a word. This will not do. For, in so far as this merely relieves him of the job of defending a reduction, it is a subterfuge, at any rate as long as we mean, as we do, reduction with respect to ' communication[2] by means of language ' and not with respect to an ontological position. Also, the terms in which he describes to his satisfaction the *rôle* or *use* of a word are patently of a limited kind (described above as ' concrete '), and thus there is a reduction. Finally, is it not plain that, apart[3] from their ontological views, reductionists thought that their favoured reductions constitute a clarification?

We note in passing that Wittgenstein, who is very sensitive to the misconceptions that (may) accompany traditional philosophical problems, is often peculiarly insensitive to what the traditional aims were.

With this out of the way, recall that crude behaviourism seeks to identify mental acts with particular physiological processes and larynx movements. Crude nominalism, for example with respect to numbers, identifies them with number signs. Wittgenstein's outlook constitutes a modification in at least two respects. First, he speaks freely of certain mental acts such as sensations in the usual sense, but also of seeing a *Gestalt;* [4] and of configurations with such and such a number[5] (not only of number signs), rules, etc. Second, when he comes to such mental acts as the acts of meaning, wishing, copying, etc. which he wishes to distinguish from sensations and images, he does not try to identify them with the others, but to describe their [6] rôle in our language ', and this is then done in concrete terms. Similarly, he does not identify numbers with number signs, but, again, wishes to describe what we ' do ' with them, and this too is done in the terms just outlined. It seems to me, whatever the limitations considered below, he describes in these terms, often surprisingly successfully, situations[6] which are normally considered to involve just those mental acts and abstract objects which he eliminates. By ' successful ' I simply mean that we agree spontaneously that his description expresses what we want to say. It is important to note that (justified) doubts about the exact meaning of ' concrete ' do not spoil the reduction since it is sufficient if only the terms employed in the particular cases are recognised as concrete.

[1] We shall return to the business of grammar below.

[2] The phrase occurs on p. 89. In these books, Wittgenstein is not much concerned (explicitly) with ontological questions.

[3] Of course, if one does hold a restricted ontological position, clarification will be a secondary aim, since in the first place one wishes to get rid of terms which were wrongly supposed to refer to something.

[4] More generally, seeing (hearing, feeling . . .) *X* as *Y*. See the *Remark* on p. 243.

[5] Physical realisations of an abstract object, cf. Bernays' notion of *Bezogene Existenz* in ' Mathematische Existenz und Widerspruchsfreiheit ', *Études de philosophie des sciences*, Neuchâtel, 1950, 11–25.

[6] Many of them, particularly in the Blue Book. It is beyond the scope of this review to examine them one by one.

We mention in passing that detailed investigations[1] in the foundations of mathematics have revealed a similar situation with respect to a nominalist (finitist, or, more generally, predicative) elimination of such abstract objects as the totality of natural numbers or of functions. It is understood that this is applied to *certain contexts* only, usually specified by means of formal systems. Thus many of the usual proofs in arithmetic apply the principles of classical logic to statements containing quantifiers over the natural numbers, which makes immediate sense only on the assumption of such an entity as the totality of all numbers. But, for a wide class of proofs, the so-called finitist[2] interpretations which have developed from Herbrand's famous theorem, provide an elimination in a quite precise and natural sense. Of course, just as in Wittgenstein's description of the ' rôle ' of an abstract term, one does not make a crude identification between, for example, a finitist and classical existential operator. Similarly, in a large part of analysis, quantification over all real numbers can be eliminated, and by use of the theory of hyperarithmetic operations of Kleene and Mostowski, the predicative[3] content of assertions involving such quantification is made explicit.

In conclusion, about what may fittingly be called Wittgenstein's practice of philosophy (in large parts of the Blue Book and small parts of the Brown) we may say this. Both his examples and the studies in the foundations of mathematics show clearly that *we have a general tendency to describe language* and, in particular, mathematical practice, *by means of concepts whose level of abstraction is higher than the minimum actually*[4] *needed.* As a corollary we have this: whatever the significant problems concerning the relation between the abstract and the concrete may be, they do not arise in the cases here considered. (More precisely, they do not arise vividly; for, of course, a

[1] My own in this direction have certainly been influenced by the view of Wittgenstein's work here described.

[2] A variant to Hilbert's theory of the foundations of arithmetic, this *Journal*, 1953, **4**, 107–129. This is a more thorough reduction to the concrete than might be supposed from the use of formal systems with arbitrarily long formulae, etc. For, in the interpretation of theorems that actually occur in ordinary mathematical practice, rather simple primitive recursive functionals are used, and those defined by means of transfinite induction with respect to well orderings near ϵ_0 are not needed. Similarly, in the next footnote, only arithmetical operations low down in the hyperarithmetic hierarchy are used (if the theorem permits a predicative interpretation at all).

[3] Kleene and Mostowski, ' La prédicativité ', *Bulletin de la Société Mathématique de France*, 1960, **88**.

[4] Here again: within the limited context. For, *given* the abstract conception the semi-nominalist description does *not* render the full content of the statement containing abstract terms, nor conversely. An example of the latter is the harmless statement: for all natural numbers a and b, $a + b = b + a$. For, the meaning of a quantifier is different on the finitist and the extensional interpretation, the former implying that we have a proof of the statement. However, in the particular case, we may have to agree that either interpretation is consistent with our intuitive meaning on some given occasion.

successful elimination of certain occurrences of abstract terms no more excludes all need for them than for example the successful application of continuous mechanics excludes the existence of atoms; even in such cases one can ask for the relation between macroscopic and atomic laws. But this problem is less vivid than where the macroscopic laws go wrong.)

In connection with this conclusion we note for use in the next section a peculiar fact about the practice of mathematicians: they hardly ever use even that level of abstraction which is needed to give the ' interpretations ' mentioned above. Thus, though they formulate the general principle of induction in number theory, which would allow them to be non-constructive, practically all number theory is constructive. In analysis, they are non-constructive, might be impredicative because the principle of the least upper bound is so, but practically never are. In set theory they are, but do not use axioms of universes efficiently. Their real problem is not at all to get rid of the abstractions but to learn to use them.

Remark. I think this section of the present review does justice at least to the earlier portions of the Blue Book, but it may be too logically biased and even altogether too pragmatic. A remark in Bernays' review (see p. 240, n. 3) suggests a completely different point of view for discussing the later portions, namely a phenomenological (as against a logical) bias. In old fashioned language, we may look at these books, particularly the *Brown Book*, as a contribution to the study of *what is concrete*, of *what is* (*immediately*) given. Further, we should expect these books not so much to give us a theoretical understanding, but rather to make us aware of what is given. Wittgenstein's writing is so vague that it is unprofitable to look for contradictions, but there is certainly a world of difference in what he thinks[1] of introspection on page 18 (in connection with the theoretical question of the existence of sense-data) and on page 164 (in connection with seeing *X* as *Y*). Without going into a discussion of the latter phenomenon, so much at least is clear: if one is out to ' chart the concrete ' and notices this phenomenon for the first time, one will be as excited as an astronomer who finds a new planet. Now, it seems to me clear that such phenomenological studies are of interest in themselves and basic to other approaches in philosophy. For instance, in the reduction described above, the very choice of the primitive notions is, implicitly or explicitly, determined by an interest in what is concrete. Also, for example, the attributes of certainty and clarity, sometimes associated with propositions about sense-data or combinatorial facts (in contrast to material or abstract objects), should surely be analysed here with respect to ' quantity ' of concrete content; for, certainty in the sense of improbability of error, or clarity in the sense of exactness cut across the two kinds of propositions described above. But, I believe, though even

[1] In fairness one should add: the actual irrelevance of introspection to the issue on p. 18, and is central rôle for the problems of p. 164.

in his later book *Philosophical Investigations* these phenomenological studies have not gone far enough to establish a discipline, the later work is incomparably better in this respect than the books under review. So it seems unprofitable to discuss this any further here, even if I felt competent to do so.

Wittgenstein's theoretical positions. Here I do not mean the little homilies on the aims and purposes of philosophy mentioned in the introduction above, but (i) negative assertions on what cannot be said (or: is not), such as what is common or essential to those cases which he describes as families[1] of concepts, (ii) assertions on what should be accepted as a decisive criterion for (equality or difference in) meaning, such as the actual use of a term, (iii) the identification of metaphysical distinctions with grammatical ones. I call these ' theoretical ' because they constitute a basis or at least directives for a ' descriptive philosophy ' (e.g. what it should not attempt to describe). They certainly are not summaries of investigations, but much more pronouncements on their significance; for instance, in case (i) when he does not say what is common to all instances of a general term, the significance of what he does do (by way of examples) is of course greater if there is ' nothing left to be said '.

To analyse what is common to all instances of a general term in informal use, is one of the traditional[2] problems of philosophy. In its weaker form, it may be construed as requiring necessary conditions or ' what attributes apply to . . . ', in a stronger form as requiring necessary and sufficient conditions or ' which (well defined) attribute applies to . . . '. Wittgenstein objects (*a*) generally, to the introduction of an (abstract) object common to all instances of a general term, (*b*) to the assumption that a general term always corresponds to a (single?—presumably: well defined) property. He says (p. 19) that (*a*) has led to no results. Before going into the discussion it is well to remember the results of the last section, namely the tendency to introduce a level[3] of abstraction higher than is actually needed. Thus when one looks for examples, the first ones that come to mind will not use the level in an essential way. Another point to remember is that (*a*) is evidently trivial if one starts with the doctrine that all meaningful formulations must be expressible in concrete terms (and, of course, highly plausible if one holds this doctrine without saying so). So, in particular, in many

[1] The metaphor of ' family likeness ' suggests to me almost the opposite to what is intended. We analyse family likeness in terms of *genes* which, at any given time, are supposed to have a definite effect, and account for variations over a short period by the environmental changes (thermal agitation, etc.) and over a long period by mutations.

[2] Wittgenstein speaks more about the question of ' what is common to . . .' in the earlier portions, presumably because later he became engrossed in his own problems.

[3] In the last section, the levels considered were mentioned explicitly (constructive, predicative, . . .), etc. In the general case it is useful to employ a correct, but very crude measure, namely the highest type of object, in the sense of the simple theory of type, employed in the course of the argument considered.

cases of (*a*), especially if the general term is applied to concrete situations, he is completely in accord with (what I conceive to be) the Russellian tradition: for example by identifying *The Blue* with the class of blue objects[1] one ensures, by the no-class-principle, that the abstract term is eliminable from all meaningful contexts; in other words, if one absolutely wants to, one can have it, but it is patently not necessary. In the case of *The Blue* this is probably so except perhaps in some theoretical statements in (the) phenomenology (of colour). As to (*b*), in many[2] cases everybody would agree anyway.

The real issue, I believe, is this. In case (*a*), when we ask for what is common to certain formal properties or relations, for instance what abstract structure is common, for example to rotations in the plane and multiplication of complex numbers. In case (*b*) when it is evident that the property could not conceivably be expressed in concrete terms, but may be formulated in abstract terms; for example this would apply to ' mechanical procedures ' (which Wittgenstein calls a family) where one uses the non-constructive existential quantifier in the definition of recursiveness.[3] More explicitly, the issue in (*a*) can be put as follows: ' Why this mouthful about " abstract structures "?[4] What one shows is that formulae of a certain shape, instances of a formula schema, are valid in both cases (so-called group laws) and then one formally derives consequences from this. Of course, the notion of validity is used, but that, after all, can be expressed by: these formulae are formal consequences of axioms for rotations and complex numbers respectively.' Or, a little less (more?) brashly: ' Well, there *is* a conceptual distinction, but it's a distinction without a difference.' I think we are back to note 4, page 242. The distinction is simply not vivid unless there is a patent difference; but, somewhat reluctantly, I am persuaded that it is real, by rather recent developments in mathematics where set theoretical operations are applied in a highly impredicative manner: they seem evident only on the conception of an abstract structure.[5] The question is of course

[1] Images would be regarded as objects here on the outlook of the last section.

[2] Cf. Bernays' remark on *The Good*, loc. cit., footnote 10.

[3] I use here the ' family of concepts ' of mechanical procedures because Turing's argument seems to me rather stronger for this case than for (humanly) effective procedures.

[4] The strong opposition of some mathematicians to the Bourbaki conception of mathematics (and Bourbaki's self doubts in the introduction to their volume on set theory) could be phrased similarly. It is to be noted that its aims are just about opposite to those described in the previous section which lead to reductions.

[5] Note that these cases are not ' too general ', because the really general conceptions treated in philosophy such as proposition or property are definitely of a higher level of abstraction still. Thus to formulate the general notion of a well-defined property P (= for every object b either P applies to b or P does not apply to b) we need a non-classical interpretation of the logical constants which is meaningful when applied to expressions which are not necessarily either true or false, since otherwise the condition in brackets would be empty. For a coherent and, in my opinion, very promising approach see the work on type-free logic by Ackermann and Schütte, for example, *Math. Zeitschrift*, 1954, **61**, 160–179.

not settled if one regards this latter evidence as problematic. I believe the situation is not at all clear, but, at least, here we have an area where the issue is vivid. As to (*b*) it is unavoidable to refer to detailed discussions: for example, concerning mechanical procedure, the discussion by Turing[1] or a more recent (and far less satisfactory) note[2] concerning finitist proof. It seems very natural that one is not instantaneously convinced of correct characterisations even if the arguments are good on reflection. After all, we do not have much experience with these matters, and it would be amazing if our intuitions (first thoughts) were to be both reliable and confident: the degree of conviction carried by the same experiment in the natural sciences or the same argument in mathematics certainly changes with the development of the subject. (This is, of course, a corollary to the familiar assertion that standards of rigour are currently established in the course of developing a subject and not before.)[3]

The popular response to the question of what is essential to a concept (again, primarily in the sense of 'necessary' and not of 'necessary and sufficient') is, of course: for what? This rather harmless piece of philosophy would naturally be followed by investigating whether there is something that is essential for all its applications or the huge majority of them. Thus we should certainly assert that transitivity (if $a = b$ and $b = c$ then $a = c$) is essential to the concept of equality. We should regard deviations[4] from this in actual uses of equality (e.g. visual comparisons) as inessential or as incorrect, depending on further reflection on the cases involved; certainly, unless one starts with the doctrine that all details of actual usage are decisive, there is no inconsistency in this. Returning to note 5 on page 245, present day mathematics has certainly gained a great deal in clarity by the rather surprising discovery that relatively few abstract structures were essential to the proofs in the greater part of current mathematics. (Here, the emphasis is not on the abstract structure as an object.[5]) In any case,

[1] A. M. Turing, 'On Computable Numbers, with a note on the Entscheidungsproblem', *Proc. London Math. Soc.*, 1936, **42**.

[2] *Proc. Int. Congress of Math. Edinburgh*, 1958, 289–299. This should be read in conjunction with the clear philosophical arguments in Gödel's contribution to the Bernays Festschrift, *Dialectica*, 1958.

[3] Incidentally, the same applies to the delineation of the subject matter itself. Hence the futility (at the present time) of pressing too hard the question: 'What is philosophy?'

[4] On p. 125 Wittgenstein compares the search for essentials of a (different) term with trying to find the real artichoke by stripping it of its leaves. I am sure he confused artichokes with onions. But what he actually says, fits our case much better. There may be some indefiniteness about the extent of the core of an artichoke, but the distinction between core and leaves is real (and essential).

[5] We may note that a proof of the validity of a theorem in such and such an abstract space is often important not because of the extended applicability of the theorem but because it is an answer to the question: What is essential to the theorem?

in so far as ' putting things in order ' (pp. 44-45)[1] is concerned, it is surely as helpful to find a precise concept from which there are few deviations as to point out differences, and differences between differences, etc.

As far as *actual use* of words is concerned, I am in the unfortunate position (in view of the last paragraph) that I have to begin with a distinction. ' Actual use ' may refer to the words spoken: this has the attraction that here we have a subject matter for philosophy comparable to the ' hard ' experimental facts of the natural sciences or the combinatorial facts of mathematics to which one can refer when the theoretical framework creaks. Furthermore, since it includes everything that is said, it would seem to leave room for all things between Heaven and Earth. But ' actual use ' may also mean the *real* rôle of the word (as Wittgenstein puts it), undistorted by the vagaries of linguistic expression from which philosophy should free us: this has the attraction that now we are getting real knowledge. But, unfortunately, the latter is, in general, achieved at the cost of the former. Now, I believe that in the cases of elimination of abstract terms described as successful in the last section, there seems no doubt about the actual use (in the second sense), just as one does not doubt that, in the future, the very simple observations of chairs and tables will continue to be taken as ' facts '. But in other cases the whole problem is thrown back to what is conceived as the real rôle (cf. *RFM*, p. 152, 8.3*b*).

On page 137 of *RFM* (1.3*a*) I also questioned the value of the ' reduction ' of metaphysics to grammar. Of course, the attraction here is again (to use his own expression) that the occult is eliminated and replaced by ordinary everyday words. This was, indeed, also the attraction of the early syntactic approach to logic, where the syntactic rules (for operating with the formal expressions) were of an elementary character. But then, I believe with Carnap, the rot set in: simply because the objects talked about were symbols, for example, the notion of a set of formulae true under such and such an interpretation, was called ' syntactic '.[2] This notion is no less problematic than the notion of ' truth under the given interpretation '. The reduction to grammar seems to suffer from a similar defect: I see no evidence that the grammatical distinctions which are to replace (problematic) metaphysical ones, are going to be described by means of less problematic concepts. More specifically the reference to grammar is deceptive for at least two additional reasons: First, perhaps for historical accidents, one does not usually consider such questions as ' what is a noun ' in a theoretical way; the question is undoubtedly grammatical, and hardly more tractable than ' what is an

[1] It is revealing (top of p. 45) that Wittgenstein's stated aim is to *separate* the books. The metaphor could be developed: if the room (= powers of memorising) were very large, one could just put every book by itself, and contemplate it in isolation.

[2] There are certain theorems in algebra which are fully formulated only if one considers such sets of formulae. This is of course no reason for calling these sets ' syntactic(ally defined) '.

object'. Second, while it seems to me perfectly apt to speak, as Wittgenstein does, of the grammatical rôle of a word in a language, the difficulty of formulating this seems to be of an entirely different order from school grammar where one classifies words into categories often even independently of their position in a sentence. Without wishing to press the analogy, one is reminded of the difference in the conceptual problems involved in simply weighing things and in formulating the relation between mass and weight in terms of the distribution of masses in the universe. In short, grammar could be very difficult.

However, it is to be emphasised that in so far as the 'cult' of actual usage or of grammar is objectionable this is not so because these notions are imprecise in places or present problems of their own. But rather because such problems seem to arise just at the very juncture where the 'metaphysical' concepts which they are to replace, present serious problems.

Remark. The present section, which is almost entirely critical, may convey the impression that no theoretical positions of any generality, in particular no impossibility or undefinability assertions, can be established. On the contrary, I believe that some of the *most outstanding results are such negative results, only Wittgenstein's approach is particularly unsuited to such work.* In particular, their significance is connected with the fact that Wittgenstein's general theoretical positions are wrong:[1] Suppose the impossibility of a characterisation by certain means (e.g. mechanical procedures) is to be shown, where the means considered form a family of concepts in Wittgenstein's sense. Suppose further we find an abstractly formulated property (here: recursiveness) which is certainly satisfied by all members of the family, and possibly by things outside it. *If we now establish the impossibility of achieving the required end by all methods which have the abstractly defined property, then we have a negative result which is unaffected by uncertainties* about the exact extent of the family of concepts considered. (It is of course not even required that the 'family' should have an exact extent.) I believe the epoch-making character of the work initiated by Gödel rests largely on satisfying all these suppositions (for details, see *RFM*, p. 154). The possible need for an abstractly defined property is also apparent here; namely if all the well-defined properties which are common to such a family (and sufficient to derive the required conclusion) are definable only on a higher level of abstraction. Now, if one rejects the use of abstract concepts as a means of philosophic analysis (or, at least, considers an explanation in concrete terms more fruitful) one will tend to reject the particular interpretation just discussed. Since this interpretation is certainly natural it seems under-

[1] In *RFM*, p. 153, I stressed the wildness of Wittgenstein's arguments in connection with Gödel's work, but left open (line 8) the source of this. The analysis below may be a partial answer.

standable why Wittgenstein objects so strongly to attributing philosophic significance to the impossibility results of mathematical logic.

It may be worth while to add at this point a comment on the use of mathematical logic in philosophy.[1] It is certainly true that some ambiguities with ' and ' and ' or ', for example, the negation of: *A* or *B*, and ' all ' and ' some ' are resolved by the current logical notation. But the case for this seems hardly different from teaching Latin as an aid to good English: a clean intellectual home (*gute geistige Kinderstube*) can look after both. On the contrary, emphasis on this point detracts attention from the places where mathematical logic appears *essential*, either *for deriving results as above or even for formulating*[2] *a philosophical, for example an ontological conception*, sufficiently sharply to draw conclusions. The latter is not surprising if one remembers that often a physicist's conception can be formulated only by the use of mathematical machinery not known to him, for example Maxwell's theory for Faraday's conception. Just as in physics out of the way matters are sometimes more tractable[3] than what is familiar, open to inspection and experimentation, so, here, we have to take as an example two ontological positions in ' higher ' mathematics, namely these: (*a*) the natural numbers are abstract objects which exist independently of us, (*b*) we create them. Wittgenstein objected to (*a*), essentially because (*a*) had ludicrous associations for him, and probably would have found something wrong with the formulation of (*b*). (These are the philosophical positions referred to on p. 241.) Without any mathematics, one can make the contrast between (*a*) and (*b*) vivid by reference to the irreverent conundrum: Can God create a stone so heavy that he does not know how to move it? Now, if the totality of possible stones and blueprints for moving them were fixed, this question would have a definite answer in either of the two natural (though different) senses: (i) does there exist a stone that defeats all blueprints or (ii) does there exist a blueprint that defeats all stones. However, if these totalities grow the answer would, in general, change from time to time (even for a given sense) according to whether stones or blueprints are getting the upper hand. Finally, there is the situation in between where an unreflective creator assumes in ignorance the second possibility to hold, while a shrewd guess

[1] This is a strictly personal opinion, shared perhaps by some logicians with a mathematical bias but for example not by the majority of even those professional philosophers who strongly support mathematical logic in a philosophical education.

[2] This is certainly denied by Wittgenstein, and his position, in turn is criticised in *RFM*, p. 143 (3.2). However, mathematicians, too, shy away from the responsibility of having something to contribute to ontology (or physics, for that matter), cf. Fränkel Bar-Hillel, *Abstract Set Theory*, North Holland Press, Amsterdam, p. 344.

[3] Thus in the seventeenth century one could say informatively why the moon has such and such a period (given its distance from the earth), while one had only a futile linguistic answer to: why is glass transparent?

about his methods shows that he has reached a steady state (with respect to the present issue). In consequence, a ' definition ' of an object *a* by the condition: *a* is the oldest unmovable stone if there is one and the oldest stone if there is no unmovable stone, certainly singles out a unique stone if the totality is fixed, does not in the second case, and may do so in the third. This already brings us fairly near a mathematical formulation, namely this: If (*a*) holds then, whatever else (*a*) may mean, quantification over the totality of all numbers has a well defined sense[1] and classical logic may be applied to statements so formed. On (*b*) this is certainly not justified, and on the strict interpretation, namely the second and not the third possibility above, it would not make sense to use quantifiers. But, if one asks, whether a given statement essentially presupposes (*a*), in the sense that it could not be proved by means of principles which are evident on (*b*), then it is clear that a mathematical characterisation is needed for an answer. If (*b*) is interpreted in the strict sense, we get a rough illustration of what is meant by a finitist proof. It is to be observed that one is not giving an *arbitrary* technical meaning to the rough conceptions (*a*) and (*b*). There is certainly no virtue in using mathematics in order ' to be precise ' at all costs. The unmotivated axiom-mongerer in philosophy is on a par with the applied mathematician who ' sticks in another parameter ' to fit his curve.[2]

Summary. I think an explanation has emerged of the two striking facts noted at the beginning of the review.

(i) When dipping into the books one is offered imaginative examples pointing out some ' blind spot ', surprisingly concrete reformulations of what appears at first to presuppose higher levels of abstraction, and striking, but easily verified phenomenological facts. The latter are all the more attractive because Wittgenstein is evidently genuinely interested in them, and not only as illustrations of a general thesis. There is (I believe) no particular inherent difficulty in the points made or strangeness in his point of view. Also, at the beginning, he sets himself rather clear problems and aims. But disappointment sets in when he begins to waver, evidently goes in a new direction, *but never returns to re-examine the earlier positions.* Also, one is put off by the misconstruction and misinterpretation of evidently significant problems.

(ii) No doubt, one is inclined to have higher standards in one's special field. But, I believe, we also have the following objective fact: the concrete content of mathematical assertions is far less significant than of the situations described in the books under review, in all but the most elemen-

[1] More generally, the comprehension principle in the theory of types. This clarifies the footnote on p. 138 of *RFM.*

[2] The latter is despised by the physicists. This seems to show that they do not share completely Boyle's view that error is less damaging to progress than vagueness.

tary parts of mathematics such as the addition and multiplication of (small) numbers.[1]

As to content, the ideas of the book seem to be most relevant to the discipline which studies what is concrete (and whose exact delineation is yet to be evolved). On the positive side there are descriptions of little noticed phenomena (phenomenology) and reductions to concrete terms of many situations that are in the first place viewed abstractly. As described above a wider sense of ' reduction ' is appropiate than is used in crude positivism or nominalism. This work shows convincingly a natural tendency of being unnecessarily abstract. On the negative side we have Wittgenstein's theoretical positions; on analysis, they are seen to be cogent consequences of philosophical doctrines which, roughly speaking, overestimate what can be done in concrete terms. Since the former seem to be easily refuted they are useful in *reductio ad absurdum* arguments applied to the latter.

As an introduction to the significant problems or traditional philosophy the books are deplorable.[2]

G. Kreisel

Correction to RFM. On page 153 (9.1) of *RFM* I asserted that Wittgenstein's remarks on the ambiguity of the notion of an enumeration of (number theoretic) functions were unfounded. This assertion was correct because he was explicitly concerned with Cantor's set theory where a function f is identified with the set of pairs $< n, f(n) >$, and the notion of enumeration is unambiguous. However, his remarks can be given a little more sense if an intensional notion of function (*rule* of calculation) is considered. If we take the familiar notion of a rule of calculation expressed by a set of (recursion) equations, at least three meanings of enumeration suggest themselves:

[1] Wittgenstein attempts to analyse their ' concrete content ' elsewhere, loc. cit. note 2, p. 238 above. In particular, he recognises and emphasises the problem of analysing the concrete content of such simple operations as *copying*. It may well be that the approach in the books under review may help (some people) with this sort of problem. Personally I suspect that just these basic operations will require for fruitful analysis a very sophisticated conceptual apparatus (cf. the problem why is glass transparent, in note 2, p. 249 above).

[2] This is largely based on a personal reaction. I believe that early contact with Wittgenstein's outlook has hindered rather than helped me to establish a fruitful perspective on philosophy as a discipline in its own right, and not merely for example as methodology of highly developed sciences. In fairness (I do not know whether to him or to me) it should be added that most philosophy of the day including consciously anti-Wittgensteinian brands, does not seem much better. Exceptions are, in my opinions, the philosophical writings of Bernays and Gödel, some of which are quoted in the text, but also for example the latter's contribution to the Schillp volume on Russell, which is not. The usefulness of the ideas of these writers is, at least in my opinion, not confined to the area of mathematical philosophy.

(1) an enumeration of the symbolic expressions of the rules, that is, of the equations, in which distinct equations get distinct numbers; (2) one in which distinct rules which define functions with the same course of values, get the same numbers, others get different numbers; (3) one which associates numbers not with the symbolic expressions of the rules, but with (suitable finite) sets of values of the functions defined by means of such rules. Now, (1) is evidently possible, (3) is trivially impossible since the number associated with a function depends only on a finite set of its values, (2) is seen to be impossible by means of a not altogether trivial argument,[1] if we require the enumeration to be given by a rule of calculation too. And even though non-enumerability is established both in senses (2) and (3), these senses are certainly different.

Causality—The Place of the Causal Principle in Modern Science. By Mario Bunge.
Harvard University Press; London: Oxford University Press, 1959. Pp. xx + 380. 60s.

THE nature and extent of causal relations and their bearing on determinism are perennial questions in the philosophy of science, and the variety of modes of causation in science which now have to be considered has made many traditional discussions of the subject obsolete. We must therefore be grateful to Professor Bunge for this detailed survey of causality and determinism in relation to the various kinds of theory that appear in modern science, and for his criticisms in the light of these of many traditional views. Thus, he questions the commonly accepted reduction of causality to invariable succession or to predictability; the view that functional laws may in themselves express causality; the view that causality is valid at least throughout *classical* physics; and the positivist assertion that the progress of science brings unification of explanatory concepts and theories. On the last point he makes the very pertinent observation that, on the contrary, science shows progressive *differentiation* of concepts and laws—an observation which escapes us only when we become so impressed by the *formal* success of unified mathematical theories that we forget that these have to be interpreted in widely different empirical situations, and that every such interpretation brings its own conceptual complexity.

Differentiation is in fact the keynote of the book. There is, Bunge argues, not just one problem of the nature of determination in science, but many modes of determination of which causality is only one, and which

[1] Partial recursive functionals and effective operations, *Constructivity in Mathematics*, North Holland Press, Amsterdam, 1959.

also include law-like relations which he characterises as structural, statistical, teleological, and assertions of class-inclusion. He defines *determination* in terms of three necessary and sufficient conditions: the *genetic principle* ('nothing comes out of nothing or passes into nothing'), the *principle of lawfulness* (that there are regularities of events), and the *ontological interpretation* of the laws involved. Causality itself is that tighter form of determination which implies a one-one relation between two events C and E such that 'If C happens, then (and only then) E is always produced by it.'

The distinctions here made and their applications are illuminating and are worked out with copious references to the science and philosophy of the past and present. Many points of detail deserve attention, but I shall confine my remarks to what is likely to be the most controversial feature of the book, namely the ontological interpretation of determination, and its reflection in the notion of *productivity* in the definition of causality quoted above.

Bunge wishes to insist, against all the heirs of Hume, that there are laws which 'work objectively . . . are immanent in things, . . . are modes of being and changing of things . . . are discoverable and not inventable' (p. 250). This ontological assumption is so closely woven into Bunge's account that it is difficult to discover whether he is intending to provide specific arguments to support it. Apart from a rather sketchy quotation of an inconclusive refutation of Hume, his arguments continually *presuppose* the ontological nature of causality and determination in general, and use the presupposition to refute or to help in refuting various positivist accounts of causality. Thus the reader is left in a difficulty, for although this reviewer at least is initially sympathetic to an ontological as opposed to a positivist interpretation, it is not clear how the distinction between them can be upheld unless it is shown that it makes *some* difference to be an ontologist—not of course a directly empirical difference (no one expects so metaphysical a position to be in a crude sense empirically testable) but at least some difference that is more than a comfortable feeling of 'being in touch with reality'. The difficulty here is that Bunge seems to operate with an almost closed circle of concepts, few of which admit of any interpretation which meets the anti-ontologist objections. Thus a typical argument is the following: a necessary condition for a causal relation is said to be the *production* of the effect by the cause; only *events* and not qualities or dispositions can be productive; *states* of a system are qualities and not events; hence mere regular successions of states are not causal. But when we try to break into the circle by enquiring how we can recognise *productivity* or *event*, Bunge becomes elusive—I cannot find anywhere in the book an independent discussion of what is meant by either term.

Perhaps some light can be thrown on the positive content of Bunge's

ontologism by examining the views he uses it to refute, together with the other reasons he gives for refuting these same views, for if certain kinds of scientific relation are distinguished from others on grounds which clearly do ' make a difference ', and if these are then said to be ' ontological ' and the others not, this in itself may be made to give a sense to ' ontological '. Several of Bunge's points might be relevant here:

(1) He argues that causation cannot be reduced to predictability, since (*a*) prediction is never exact, whereas he has defined the cause-effect relation as one-one; and (*b*) ability to predict, for instance the explosion of an atomic bomb, does not entail that we have a complete causal explanation of what happens.

(2) He argues that causality is not reducible to mathematical forms of relation, since these are not directly testable, and the same form may be interpreted in many different ways, some causal and some non-causal.

(3) He argues that causal explanations in advanced science demand enquiry into the intelligible mechanism of change. Mere succession or mere logical form does not lead the scientist to feel he has understood the change, as is clear from the way in which theoretical explanations are *summarised*—the detailed logical framework is omitted and the ' stuff ' of the explanation is described, showing that it is the latter which is thought to be the essence of the explanation.

Points (1*a*) and (1*b*) are acceptable enough, but it is doubtful if the appeal to ontology can be made to rest on them. All that is shown is that the causal relation as Bunge has defined it is not universally applicable in science, and this he himself argues later in the book, where he points out examples of types of determination which are not causal. The arguments against reduction to predictability do not show that other forms of determination are not so reducible, and indeed it may be that just because of arguments such as these, it is other forms of determination that are found in science, and not causality as Bunge has defined it.

The arguments (2) and (3) seem to offer firmer ground for ontology, but unfortunately are not pursued in any detail. This is a pity, because it is more than ever apparent after reading all that is said about ontology and natural science in this book, that the most hopeful way to approach the question is by consideration of what is involved in the ' interpretation ' of a mathematical theory, what it is for an explanation to be ' plausible ' or ' intelligible ', and how far such characteristics are implied in the way laws and theories are used. If it is found that the inferences we wish to make from laws and theories presuppose a distinction between law-like connections and merely accidental connections, then this distinction itself may provide good reason for describing the one and not the other as ' ontological '. Two

investigations along these lines would seen to be relevant: one is the question of counterfactual conditionals, which Bunge curiously does not mention; the other is the question of the reality-status of models for scientific theories. In the first case, the fact that we feel entitled to make counterfactual inferences from law-like connections but not from accidental ones gives some grounds for ascribing ' ontological necessity ' to the former; and in the second case, to show that we make inferences from theoretical models in the same kinds of way that we make inferences from situations involving objects whose reality is unquestioned, gives some grounds for using the predicate ' ontological ' of explanations involving such models.

However, this is not the book Professor Bunge has written, and it may seem churlish to concentrate this review upon the suggestion that he should have written a different one. But it is impossible not to regret that so detailed and informative an account of the workings of causality in modern science has not been used to enquire into what a post-positivist philosophy of science must regard as the central problem.

MARY B. HESSE

Experience and Reflection.

By E. A. Singer, Jr. Edited by C. West Churchman. University of Pennsylvania Press, Philadelphia; Oxford University Press, London, 1960. Pp. xv + 413. 40s.

THIS book is the result of Professor Churchman's editing of a manuscript left uncompleted at Singer's death. Churchman contributes a short introduction in which he gives a summary of Singer's thesis and indicates briefly the respects in which the work is incomplete.

It is in three sections. The first part contains a discussion of traditional theories of knowledge under three main headings. According to Singer, Rationalism holds that ' No knowledge of law implies knowledge of fact; all knowledge of fact implies knowledge of law ', Empiricism holds that ' All knowledge of law implies knowledge of fact; some knowledge of fact does not imply knowledge of law ' and Criticism, in the Kantian sense, holds that ' Some knowledge of law implies, and some does not imply, knowledge of fact; some knowledge of fact implies, and some does not imply, knowledge of law '. But there is a fourth possibility, namely that knowledge of law depends on knowledge of fact *and conversely*. One aim of the book is to examine this possibility in relation to physics and biology. This purpose, although implicit throughout, seems sometimes to be lost sight of, no doubt because of the incompleteness of the manuscript.

The second part consists mainly of a consideration of the nature of scientific questions and answers in relation to physics and contains discussions of ' the given ' and of the mutual adjustment of data and ' formal images ', that is, models, which are involved in theories. It is in the light of his account of scientific questions and answers that Singer takes scientific procedures to show that knowledge of law depends on knowledge of fact, *and conversely*.

The third part concerns the ' biocentric ' sciences and proceeds, in the belief that the properties of living things cannot be entirely subsumed under the laws of physics, to discuss the construction of biological concepts which are independent of physical imagery but which never conflict with it. For this purpose, Singer attempts to define ' living body ' not in terms of structure, nor entirely in terms of nutritive and reproductive functions, but in terms of heredities. Toward the end of this section there are obvious signs of incompleteness and some important questions are left insufficiently discussed.

Since Singer's account of scientific questions and answers is fundamental to his main thesis I shall confine my comments to this. Science is faced with questions of fact but the conclusions of science are not answers in the form of statements of fact; they are in the imperative mood and not the indicative. This, Singer argues, is shown especially clearly by a consideration of metrical questions. For example, a surveyor asked for the size of an angle would, if he gave an *answer*, say ' The angle in question is an angle of $m°$ ', but what he in fact says is ' The angle measured is to be taken as an angle lying within the range $(m \pm p)°$ '. This, says Singer, is not an answer to the question but a *response* to it. There are three points of difference. The answer would be about ' the angle in question ', the response is about ' the angle measured '; the answer would have ' is ' as coupling verb, the response has ' is to be taken as '; the answer would give a specific value, the response gives a range. Thus the response is not an answer to the question asked and it is a command rather than a statement of fact. An answer-dictating response would be obtained if $p = 0$ in the expression $(m \pm p)°$ and this is a limiting conception which the scientist can progressively approach but never reach. Science is therefore faced with many questions which are unanswerable but nevertheless meaningful because we know what would count as answers. This account naturally necessitates a rather special account of fact, law, and the relations between them.

Whatever is to be said for Singer's main thesis, he has surely been misled by inessential features of scientific discourse and by his failure to give due weight to the contexts in which questions are asked. The scientist who asks for the size of an angle knows that the best he can expect is a range of values, just as the pure mathematician knows that for some of his questions he can expect a specific value but for others only a range. Otherwise, we suppose

them to be ignorant of the power and limits of the methods at their disposal. It is surely more plausible to regard the scientist as asking for a range of values, even when the words of his question do not indicate this explicitly, than to suppose him to be asking an unanswerable question. This is what he means and what other scientists take him to mean, even if the form of his question does not convey this to the layman. Moreover, if we insist on the imperative mood in the response, it is more plausible to regard the scientist as asking what he must take to be the value he requires rather than to suppose him to be so easily fobbed off with a command when he wanted a statement of fact.

But need we insist on the imperative mood in the response? What warrant have we for choosing between the responses ' The angle measured is to be taken as an angle lying within the range $(m \pm p)°$ ' and ' The angle in question lies within the range $(m \pm p)°$ '? The behaviour of the scientist in accepting answers to his questions perhaps gives us some reason to suppose that he understands them to be properly stated in the second form. Scientists sometimes do take their conclusions to be answers to their questions of fact and if our account fails to allow this we fail to understand what *they* mean by ' questions ' and ' facts '.

P. Alexander

Greek Culture and the Ego. By Adrian Stokes.
Tavistock Publications Ltd., 1958. Pp. 101. 15s.

European civilisation was formed essentially during the Golden Age of Greece. Science was born then; but art as well was given its Western mould at that time. It is of great interest, therefore, to investigate the forces at work in the classical period. By interpreting Greek art and finding out how it came about and what it means, we may also gain insight into science.

Adrian Stokes has taken the psycho-analytic approach to explain Greek culture, following Freud and Mrs Klein. Freud himself had applied his theory to such problems, though his concern was mainly with primitive cultures. H. Kelsen (1946) was the first to turn his attention to the Greeks with the purpose of elucidating scientific conceptions; his viewpoint, however, was that of the sociologist. Professor Dodds, in his *The Greeks and the Irrational* (1951), showed how much infantile phantasy remained alive underneath the clear rationality of the Greeks. Recently (1957), in a lecture on ' The Origins of Science ', I tried to show that the development of Ionian philosophy, from Thales' element of water to the atom of Demokritos, is similar to the way in which the human infant gains knowledge about the real world outside of him.

Mr Stokes begins with discussing a key concept of the Greeks—

καλὸς κἀγαθός—the idea that whatever is beautiful is also morally good. Greek art helps us to understand this statement which is often regarded as either obscure or trite. The Greek nude, for example, still appears to us not only as beautiful, but also as a realistic representation of the female body. If we look at the art immediately before the classical age, say, of Mycenae or of Egypt, we are struck by the fact how life-like Greek art is in comparison to its predecessors. The simple and clear outline of a Greek temple is another example showing that the classical mind was relatively free of the distorting phantasies which rule the more primitive 'barbarians'. This rationality was achieved by a great effort of repression which, as Freud explained, is always needed in order to build a civilisation. We have to renounce the immediate satisfaction of primitive desires and learn to sublimate them, so that energy becomes available for other purposes. Thus the infantile fears and phantasies are defeated that prevent us from seeing reality as it is.

Greek rationality is reflected in their morality. The Greeks believed in knowledge, of themselves as well as of the external world. *Γνῶθι σεαυτόν* and *Ἐπισιήμη ἐστὶν ἀρετή* were their slogans. They preached moderation in all things—*Μηδὲν ἄγαν*—and they abhorred mania—*ὕβρις*. They felt that a happy man is likely to be a good man, and that happiness and beauty go together: they strove for harmony (a Greek word, after all). Finally, some of the philosophers, like Xenophanes, recognised the Olympians to be a human invention, a projection of their own fears and wishes. In short, the Greeks believed in Man rather than in the gods. This attitude of balance and tolerance demonstrates the emotional maturity which they had achieved at the height of their civilisation. Or, to put it into psychoanalytic terms, instead of an authoritarian ego-ideal, the Greeks adopted what Adrian Stokes calls the ego-figure. While the ego-ideal arises from a primitive super-ego, the ego-figure stands for the integrated and adult personality. This is expressed in Greek art: the emphasis is on the whole and sound body. The tyranny of the super-ego is easily seen in the persecutory feelings that rule primitive society and can be found in early Greek myth, e.g. the Erynnies. The ego-figure arises more from the depressive anxieties that are aroused in us when we try to repair damage and bring together good and bad. 'The concept of beauty,' Stokes writes, 'projects, not the ego-ideal, but the ideal of an integrated system.' This is, once more, the balance that characterises the adult attitude.

A high level of creative achievement and of moral understanding was reached during the classical age but, alas, it did not last very long. Under the impact of military defeat and with the subsequent influx of Eastern religion, Greek civilisation crumbled; and the Greeks themselves regressed to a more primitive state. Until our day, they have not again produced either science or art.

The concept of ego-figure which Adrian Stokes proposes is, I think, very powerful and suggestive of application. Not only in art, but in science as well, integration is what we aim for; an instance is the integration of knowledge in a theory. Our ' body ' of knowledge must equally be whole and sound.

The philosophy of science cannot be divorced from the history of science. If we want to understand our basic conceptions, their power and their limitations, we have to investigate not only their logic, but also their origin. Burnet remarked once that science is ' thinking about the world in the Greek way '. This book by Adrian Stokes helps us to understand the Greeks better and, thus, also science.

ERNEST H. HUTTEN

Darwin's Place in History. By C. D. Darlington.
Basil Blackwell, Oxford, 1959. Pp. ix + 101. 9s. 6d.

THIS book, seventy-four pages long and with a bibliographical appendix of some twenty-five pages, in the words of the author, is an account of the ' issues that matter most in the work of Darwin, of their origins before his time and of their effects today '.

In particular an attempt is made to answer the following four questions :

(1) What was new in the ideas contained in the ' Origin ' ?

(2) Who was responsible for them?

(3) What effect did they have at the time?

(4) What do they mean for us now?

The author also claims that the quotations ' put together (for the first time) the basic documents of evolutionary theory ', and that the ' bibliography should enable those who wish to go further to follow the inquiry to its limits '.

Despite the author's disarming comment that his book is only a ' slight account ' of the questions asked, it must be said that he has not done justice to the essentials of the historical problems concerned with questions (1) and (2). For example, in his account of the history of the idea of evolution, after mentioning Leonardo da Vinci, Montaigne, and Descartes, he gives a reasonably full (for this book) description only of Erasmus Darwin's views on evolutionary theory.

Now the erroneous assumption underlying Darlington's account of the history of the idea of evolution is that this idea is one of those with a reasonably clear cut origin. In fact the idea of evolution rests on a number of other important notions, e.g. change, development, progress, time, etc., each of which has its own long history, and all of which have to be brought together in order that the major idea can be worked out.

This criticism that the author's view of the history of the idea of evolution is over-simple equally applies to his account of the history of the theory of natural selection, whose basic notions are not only of considerable antiquity but are derived from diverse sources other than biological ones.

The outline of the vicissitudes of the ideas used by Darwin during the first quarter of the nineteenth century is of considerable interest. In particular, the rough handling by orthodox thinkers of W. Lawrence after the publication in 1819 of his evolution-tainted *Natural History of Man* was a lesson not lost on Darwin. It undoubtedly made him very hesitant in publishing his own work, and it was not until the arrival of Wallace's essay that he was pushed into publication.

One of the most fascinating aspects of the history of Darwinism is that of Darwin's own gradual retreat from his Theory of Natural Selection to the view that the inheritance of acquired characters was the principal cause of evolution. Darlington's account of this and of Darwin's ambivalence in having to face the crucial issue of applying his theories to Man himself is done without the usual reservation found in most books on this subject.

The final chapters on the importance of Mendelian genetics or Darwinian theories and the wider application of these theories to other subjects are more convincingly sketched in than the rather thin earlier chapters.

Finally, it must be said that the bibliographical appendix is not quite as adequate as the author supposes. To take one example, although mentioned in the text, there is no reference in the bibliography to Malthus's *Essay on Population*, nor to any of his precursors, a somewhat surprising omission.

R. P. Gould

The Principles of Science. A Treatise on Logic and Scientific Method.
By W. Stanley Jevons.
Dover Publications, New York, 1958. Pp. liii + 786. $2.98

This is a paper-backed reprint of the well-known work of Jevons which appeared first in 1874. The setting of the pages, which reproduces exactly the lay-out of the second edition (stereotyped in 1877), is presumably the result of a photographic process. In a new introduction Professor Ernest Nagel gives a sketch of Jevons's life and explains briefly his contributions to (i) the class calculus, (ii) the theory of ampliative inductions as an application of hypothetico-deductive method, and (iii) the theory of probability. He makes it clear, of course, that in these three fields Jevons elaborated the work of Boole, Whewell, and Laplace, and he points out that Jevons's development of what he inherited was not in all respects perfect. But he rightly stresses the point that the attempt to bring together the three notions mentioned above was an important step in the history of the philosophy of science.

Perhaps no one since Leibniz had thought of all these topics together. We now know that a formal logic without quantifiers is inadequate for the purposes of science, and we are properly sceptical about the thesis that ampliative induction can be justified by a probabilistic argument from the principle of indifference; but it is evident to us that a really comprehensive book about the principles of science would have to include all the topics Jevons discussed. On the other hand, as Professor Nagel remarks, it is rather curious that Jevons did not take note of developments in the philosophy of geometry about which he might have learnt from Clifford, and that, unlike John Stuart Mill, he did not consider the peculiar problems of the social sciences in which he was himself a distinguished practitioner.

In their advertising material the publishers draw special attention to Jevons's account of the logical machine which he exhibited to the Royal Society in 1870. It is pleasant to record that the machine is now safely housed in the Museum of the History of Science at Oxford, intact except for the catgut by which its moving parts were originally connected. If computers acquire from their creators a disposition to ancestor worship, we may some day see them queuing in Broad Street to pay their respects to this simple forefather of their race.

WILLIAM KNEALE

Roots of Scientific Thought. A Cultural Perspective.
Edited by P. P. Wiener and A. Noland.
Basic Books, New York, 1957. Pp. x + 677. $8.00.

THIS volume consists of a collection of essays written by thirty scholars. The essays range over the entire history of science, from the ancient Greeks to the contemporary cosmologists. The book is divided into four parts covering the main periods of development, i.e. the classical heritage, from rationalism to experimentalism, the scientific revolution and, finally, from the world-machine to cosmic evolution.

There is a great deal of interesting material to be found in this book, though most of it is not new and also the approach to it is conventional. It is the usual biographical treatment that many of the writers apply to their theme; they describe what the Greek philosophers or Galileo, Gilbert, Copernicus, Kepler, Bacon, etc. have said. Occasionally, a writer takes a wider, more comprehensive, view. Koyré, for example, investigates why Galileo accepted certain tenets of Platonism in his fight against the Aristotelians. Zilsel describes the genesis of the concept of scientific progress. The article by M. G. Evans on Aristotle, Newton, and the theory of continuous magnitude is, I think, illuminating; he discusses how closely this theory is connected with our intuitive notion of uniform motion. Thorndyke's essay on the seventeenth century is very erudite. Toulmin's discussion of the

idea of crucial experiment as illustrated by Priestley's and Lavoisier's disagreement about the phlogiston is very relevant. Ellegard writes interestingly on the reaction of contemporary philosophers like Whewell to Darwin's theory.

The history of science is indispensable to the philosophy of science. We cannot understand our present day concepts unless we find out how they originated and developed. The editors of this volume are keenly aware that such a history of ideas is needed rather than the ' axe-grinding ' or ' dry-as-dust ' method that most writers on the subject seem to employ today. We need for the whole of science what Whittaker did for the aether theories of physics. The present book falls very short of this, admittedly high, standard. One reason is that it is written by philosophers and historians; there is no scientist among the contributors. Thus the articles are not technical enough to be of much use to the philosopher of science. However, the volume provides good information for the general reader.

E. H. HUTTEN

Physics of the Stoics. By S. Sambursky.
Routledge and Kegan Paul, London, 1959. Pp. xi + 153. 23s.

THERE are two general, basic concepts in modern physics, the concept of particle and that of field. When western science began, with the Ionian *physiologoi*, the element, like Thales's water, was not yet a very precise conception. Slowly, the two rival ideas of a continuous substratum and of a discrete particle developed from this beginning. Atomism was first on the scene, while the theory of the continuum came later. The Eleatics thought of the continuum as something static, indeed making all motion impossible. The dynamic continuum, both mathematical and physical, was in the main the contribution of the Stoic philosophers.

This is described in the present book, with many quotations from the relevant sources, and there is also an appendix giving translations of some of the texts used. According to the Stoics, the universe is filled with a continuous, all-pervading sub-stratum, the *pneuma*. It holds the various parts of the universe together and through it the forces between them are transmitted. Cohesion, *hexis*, and vital tension, *tonos*, characterise the pneuma, while the earlier views of the continuum, like Aristotle's, were merely geometrical and topological. Thus, the pneuma is the forerunner of the aether and of the field of force of modern physics.

According to atomic theory, the world consists of particles moving in the void, and the causal action between them is by impact. The central concepts are ' aitia '—originally meaning guilt and, later, cause—and ' ananke ' or necessity, which introduces the idea of natural law. However,

random events, the famous ' swerves ' of Epicurus, may occasionally occur. The Stoic conception of causality is much stricter; and so incidentally, is their ethical code. The causal chain stretches continuously throughout space and time, from point to point, without a gap. Space-time and causality so become connected as they are in the field theory of classical physics. The strict determinism makes it possible to predict the future. ' The predictions of the diviners could not be true if Fate were not all-embracing.' It is suggestive to realise that divination was the precursor of what is called induction today.

The continuum is a more difficult concept than the atom. Atomism involves the idea of the void which seems to be more amenable to treatment than that of infinity needed by the continuum theory. The paradoxes of infinity which arise from continuity were first stated clearly by Zeno of Elea. The geometrical method of exhaustion does not really solve the problems of convergence and of limit that are involved in the paradoxes. Xenocrates, who was a teacher of Zeno the Stoic, tried to overcome the paradoxes by introducing *atomai grammai*, ' atomic lengths ', smallest quantities that are no further divisible. This is similar to the modern attempts to avoid the ' infinity catastrophe ' in field theory.

The ' inwardly infinite ' character of space, as Weyl called it, that there is always still another point between any two points, however close together they are, has remained a difficulty even to us, when the continuum is physical rather than mathematical. Though calculus helps us to conquer it mathematically, the physical meaning of infinity as a process that is never completed—as a ' symbol of creation ' as Gauss called it—still requires much elucidation.

Like Professor Sambursky's previous book, *The Physical World of the Greeks*, (*vide*, this *Journal*, 1958, **8**, 347), the present volume is a mine of historical information. All our science comes from the Greeks, and so to study the origin and early development of our concepts is a necessary task for the philosopher of science.

E. H. Hutten

History of the Royal Society. By Thomas Sprat. Edited with critical apparatus by Jackson I. Cope and Harold Whitmore Jones. St Louis (Washington University Studies); London, Routledge and Kegan Paul, 1959. Pp. xxxii + 456 + 78. 50s. net.

This is a facsimile of the first edition (1667) of Sprat's famous work, supplemented by Introduction, full bibliographic and explanatory Notes, and Appendixes on the origins of the Royal Society, Stubbe's attack on the Society,

and Hooke's ' Method of Making a History of the Weather '. In its special field, as a detailed study of an important event in seventeenth-century intellectual history, the Editors have produced a scholarly and definitive work, and their notes contain many valuable references. There is no Index.

However, for the ordinary student of the philosophy, or even of the history of science, Sprat's *History*, though often cited, has little merit, being unreliable. It was written as a defence of the Royal Society against various misrepresentations, such as the charge of impiety, and as propaganda for the new empirical philosophy by an overpatriotic Divine who regarded the ' Genius of the English Nation ' as ordained to lead a Baconian utilitarian science.

L. L. WHYTE

ANNOUNCEMENT

MEETINGS OF THE BRITISH SOCIETY FOR THE PHILOSOPHY OF SCIENCE

1959

5 October: Professor A. Landé, ' From Dualism to Unity in Quantum Mechanics '

2 November: Dr J. T. Davies, ' The Simple Laws of Science '

30 November: Professor Sir Russell Brain, ' Space and Sense-Data '

1960

18 January: Presidential Address, ' An Indeterminist's View of the Physical World '

8 February: Professor R. L. Goodstein, ' The Significance of Incompleteness Theorems '

7 March: Annual General Meeting. Mr A. Koestler, ' Kepler and the Psychology of Discovery '

25 April: Professor E. Devons, ' Applied Economics: Science, Wisdom, Hunch or Witchcraft ? '

16 May: Dr Marjorie Grene, ' Adaptation and Evolution '

13 June: Professor S. Sambursky, ' Conceptual Developments in Greek Science '